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OF THE

ROYAL SOCIETY

OF

TASMANIA,

FOR

1891.

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ROYAL SOCIETY.

APRIL.

The first monthly meeting of the session of 1891 was held on Tuesday evening, April 21. The President His Excellency Sir R. G. C. Hamilton, K.C.B., presided, and there was a good attendance of Fellows. Lady Hamilton and a number of ladies were also present.

CORRESPONDING MEMBER.

Rev. F. R. M. Wilson, Kew, Victoria, was elected a corresponding member.

ANTARCTIC EXPEDITION.

Mr. **Morton** stated that he had received a list from the hon. secretary of subscriptions in aid of the Antarctic expedition, showing that the following sums had been subscribed:—Royal Geographical Society of Australasia (Victorian Branch), £200; Mr. J. S. Gotch, of Messrs. Gordon and Gotch, £100; Professor Kernot, £100; E. C. Merewether (President N.S.W. Branch of Royal Geographical Society of Australasia), £100; Royal Society of Tasmania, £50; Royal Society of Victoria, £100; Dr. Agnew, £100. He (the Secretary) had placed Dr. Agnew's name on the list as having subscribed from Tasmania.

His **Excellency** said that a few days ago he had received a cablegram from Baron Von Mueller, stating that New South Wales had placed £2,000 and Queensland £1,000 on their annual estimates in aid of the proposed Antarctic exploration. He asked whether the Tasmanian Government would place £500 on the estimates for the same purpose. He had handed the cablegram to Ministers, but had not as yet received any answer.

GIFT FROM DR. AGNEW.

The **Secretary** said that the Society was indebted to Dr. Agnew for another munificent gift. Some years ago back the Society obtained 12 parts of the Birds of Asia, by J. Gould, F.R.S., a rare and expensive work. The complete work consisted of 35 parts and Dr. Agnew had instructed him (Mr. Morton) to complete the set, which he had done through Trubner and Co., of London, at a cost of £75.

His **Excellency** referred in appreciative terms to the interest Dr. Agnew had shown in the Society, and the practical effect he had given to that interest by many valuable presents. He asked them to join with him in carrying by acclamation a hearty vote of thanks to Dr. Agnew. (Applause.)

AUSTRALASIAN SCIENTIFIC ASSOCIATION.

Mr. **Morton** said—Your Excellency and members of the Council of the Royal Society of Tasmania; having been appointed in conjunction with Mr. C. H. Grant as a delegate of the Royal Society of Tasmania to the meeting of the Australasian Association for the Advancement of Science, held at Christchurch, New Zealand, during the week commencing January 15, 1891, it becomes my duty, in the absence of Mr. Grant, who is out of the colony, to report to you on matters relating to the meeting. At the first general committee meeting, held at the Canterbury College, January, 15, Sir James Hector, K.C.M.G., President-elect for 1891, proposed that His Excellency Sir R. G. C. Hamilton, K.C.B., be the President for the fourth meeting, to be held at Hobart in 1892; a motion that was enthusiastically received and unanimously adopted. The other officers then elected for the Tasmanian meeting were Mr. James Backhouse Walker as local treasurer, and your delegate as general secretary. The New Zealand meeting, which lasted seven days, was a great success. The sections one and all were largely attended, and the deepest interest was taken in the many valuable papers read. The members attending the New Zealand meeting were most hospitably entertained by the Christchurch residents. The thanks of the association are largely due to Professor F. W. Hutton, F.G.S., the general secretary, for the New Zealand meeting, for the great attention paid by him to all members attending the meeting. As soon as possible after my return the Tasmanian Local Executive Committee, consisting of the Hon. J. W. Agnew, M.D., Messrs. James Barnard, C. H. Grant, R. M. Johnston, J. B. Walker, and myself, had a meeting with the object of taking the necessary steps to appoint Presidents, Vice-Presidents, and Secretaries to the different sections and to arrange other matters for the fourth meeting. The committee decided that the 1892 meeting shall commence on Thursday, January 7, 1892. The rules are that presidents of the various sections must reside out of the colony in which the meeting is held; vice-presidents and secretaries are to be residents of the colony. It is gratifying to know that the presidents in nine sections out of ten are already appointed:—Section A, Astronomy, Mathematics, Physics, and Mechanics. Professor W. H. Bragg, M.A., Professor of Mathematics, University of Adelaide. Section B, Chemistry and Mineralogy, Mr. W. M. Hamlet, F.C.S., F.I.C., Government Analyst of New South Wales. Section C, Geology and Palæontology, Mr. C. S. Wilkinson, F.G.S., Government Geologist of New South Wales. Section D, Biology, Professor W. Baldwin Spencer, M.A., Professor of Biology, Melbourne University. Section F. Economic Social Science and Statistics, Mr. R. Teece, F.I.A., of Sydney. Section G, Anthropology, Rev. Lorimer Fison, Queen's College, University, Melbourne. Section H, Sanitary Science and Hygiene, Professor W. H. Warren, M.I.C.E., Professor of Engineering, University of Sydney. Section I, Literature and Fine Arts, E. E. Morris, Professor of English, German, French languages, and Literatures, Melbourne University. Section J., Architecture and Engineering, Mr. C. Napier Bell, C.E. of New Zealand. Section E. is therefore the only section without a president, but His Excellency Sir William Macgregor, Governor of New Guinea, has been asked to accept the presidency of this section, and his reply is daily expected. Satisfactory arrangements have been made with all the Australian railway authorities and steamship companies for members travelling to attend the meeting. The Railway Commissioners will allow all members to travel at single fare for double journeys, and the steamship companies 20 per cent. off ordinary excursion rates. The Tasmanian Government and the Mount Bischoff Railway Co. have agreed to grant similar concessions to those granted on the New Zealand railways, viz., members to be allowed to travel half single fares during the months of January and February. The Municipal Council have generously placed the large room, and as many other rooms as can be spared, of the Town Hall buildings at the disposal of the association, and the Chamber of Commerce have consented to do likewise with their room. A large gathering from all the colonies is expected, and numerous promises of papers in different sections have already been received. From the outlook at present the fourth meeting of the Australasian Association bids fair to be the most successful yet held.

His EXCELLENCY said that when asked to become President of the Australasian Association for the Advancement of Science he at first refused, because he thought the position was one which ought to be held by someone who was distinguished in some branch of science. The matter, however, was again pressed upon him, and he was told from several quarters that it was desirable from many points of view that he take the position. It was a position which anyone might be proud to hold, and his hesitation in accepting it only arose from his thinking he could hardly be able to do justice to its duties. When he found, however, that it was the desire of the association that he should become President, he at once accepted, and having accepted, he meant to do everything in his power to make the meeting a great success. The first thing he did was to try to secure the presence of Professor Huxley, who some time ago contemplated visiting Australia. He wrote to Professor Huxley and had received a reply, in which the Professor said that he had very pleasant recollections of Tasmania 43 years ago, and it would interest him very much to re-visit the colony and compare the present with the past, but he regretted that the state of his health prevented him from accepting the invitation. He (President) was sure that they all regretted that Professor Huxley was unable to come, and that ill-health was the cause. Mr. Morton, in his report, had given a most satisfactory list of the Presidents and Vice-Presidents, and had mentioned the name of Sir Wm. McGregor as one who had not yet signified his intention to be present. He had written to Sir William, and pointed out to him the importance of the meeting. He had invited Sir William to be his guest at Government House, and he hoped he would come and lend that *eclat* which his great reputation and the great work he had done in New Guinea would bring to the meetings of the association. He had also written to two gentlemen in England, distinguished in the walks of science, asking them to come out to Tasmania and be his guests on the occasion of the meetings of the association. He hoped that both, or at least one, of these gentlemen would accept his invitation, and he had no doubt but that in the event of his being successful in announcing the names of those gentlemen as visitors to Tasmania, they would be received with the greatest satisfaction by the members of the association. He was glad to see so many present in this, the first meeting of the present session of the Royal Society, including their friend, Mr. Alfred J. Taylor, who had just recovered from a serious illness.

The RIGHT REV. H. H. MONTGOMERY, D.D., BISHOP OF TASMANIA, read a paper entitled "Some account of the Mutton Birds, or Sooty Petrels *(Nectris Brevicaudus)*, as seen in their Homes among the Furneaux Islands, Bass Straits, Tasmania, from Notes taken during a visit to the Locality in March, 1891."

Mr. T. STEPHENS called attention to some specimens on the table:—1. A specimen of shale from near the Henty River, with *Glossopteris*, etc., and closely resembling the shale in which more than twenty years before he found the first impression of this fern in the Mersey coal measures. Like the Mersey coal, this shale occurs under marine fossiliferous beds of carboniferous age. 2. Pieces of coal from the flanks of Mount Manfred, north-west of Lake St. Clair, cropping out at a height of about 3,000ft. above sea level. 3. Silurian limestone from the site of a bore recently put down at Railton in search of coal, with a portion of core belonging to the same series from a reported depth of about 600ft. Mr. Stephens remarked that the lavish expenditure which had been incurred here in boring through strata, which are well known by all acquainted with the geology of the district as being, geologically speaking, below the whole of the Mersey coal measures, was a startling instance of that enterprise of which Tasmania was supposed to be destitute. It was a pity that it was not more wisely directed.

The SECRETARY, in the absence of the author, read a paper, entitled "Notes on Mount Lyell District, Tasmania," by Mr. F. Danvers Power, F.G.S.

Mr. R. M. JOHNSTON replied briefly, when it was decided to postpone discussion until next meeting.

HIS EXCELLENCY proposed the usual votes of thanks to the contributors of papers, which having been carried by acclamation, the meeting adjourned.

MAY.

The monthly meeting of this Society was held on May 13, the President His Excellency Sir R. G. C. Hamilton K.C.B., in the chair. There was a good attendance of Fellows, and also a number of lady visitors.

THE ANTARCTIC EXPEDITION.

The SECRETARY read a telegram from Baron Von Mueller, suggesting a deputation from the Society to the Government in aid of the fund now being raised for the proposed Antarctic Expedition.

Mr. James BARNARD said that he was rather in favour of viewing this as an Australasian and Imperial question, and that the expense generally should be shared between Australia and the Imperial Government. It seemed, however, that there was no hope of that being obtained, and, therefore, he moved—"That it be a recommendation from the Society to the Council to wait on the Premier to see if the Government would be disposed to assist in this matter."

Sir LAMBERT DOBSON seconded.

The PRESIDENT said that the position of the matter was this. He received a telegram from Baron Von Mueller, which he read at last meeting, to the effect that New South Wales had put £2,000 on its estimates, and Queensland £1,000, and asking whether Tasmania would contribute £500. He had referred the telegram to Ministers, but had not heard from them. He took some blame to himself in the matter, because he had not applied to Ministers, but now that it had been proposed and seconded that a deputation wait on the Premier he thought that would be the best way to proceed.

The motion was agreed to.

CONGRATULATORY.

Mr. MORTON, as secretary, called attention to the fact that two members of the Society, Mr. James Barnard and Hon. Dr. Agnew, celebrated this month their jubilee as members of the Society. In 1841 both of these gentlemen were admitted as members, Mr. Barnard having been admitted on the motion of Mr. Bedford, seconded by Sir John Franklin. At that meeting there were present Rev. Dr. Lillie, and Captains Ross and Crozier, who had just returned from a cruise to the Antarctic Ocean. He believed that this Society, one of the oldest in Australia, was the only one that could boast of having two members who had been so long in membership, and still able to take their part as active working members. He saw from the records that Mr. Barnard had been very seldom absent from the meeting of the Society or of the Council. The only time there was a lapse in attendance was during Mr. Barnard's visit to England. He thought it his duty as secretary to bring those interesting facts under notice, and he placed before the Society the original manuscript of the election of Mr. Barnard, with the signature of Mr. Henslow, who was then private secretary to Sir John Franlkin, and acted as honorary secretary to the Royal Society.

Sir LAMBERT DOBSON expressed pleasure at hearing the interesting facts just stated, and said that all joined in congratulating Mr. Barnard and Dr. Agnew, and in wishing that they might yet be spared many years to take an active part in the work of the Society.

Mr. Barnard said he was quite taken by surprise by this recognition of his membership, and regretted that Dr. Agnew was not present on the occasion. He felt honoured by the reference made to himself. He had a feeling of shortcoming in that he had not throughout the long period referred to done more for the Society than he had done. Of course it should be remembered that during forty years he had the conduct of a very onerous department in the Government service, and had very little leisure to devote to scientific matters. He felt thankful for having been spared till this time and able to look back during all that vista of years. He remembered that the first paper read before the Society was an admirable one by the Rev. Dr. Lillie, and that Rev. Mr. Ewing prepared the first catalogue of Tasmanian birds, a work which has since been taken up and expanded very considerably by modern contributions from members of the Society. He did not, however, think he should inflict a speech upon them on the occasion, but would only again express his great appreciation of the feelings that prompted this reference to himself.

APPLE SCAB.

Mr. A. Morton drew attention to a fungus affecting the apple. During the last few weeks several apples had been sent him. On examination he found they were attacked by a fungus, and on submitting them to Dr. N. A. Cobb, of the Department of Agriculture of Sydney, he reported that the fungus was the ordinary apple scab, *Fusicladium dendriticum*, and that it might be almost prevented, at a slight cost, by spraying the trees once in every two or three weeks during the spring and summer with the mixture known as "Ammonio-carbonate of copper." Dissolve 2lb. of copper sulphate (bluestone) in hot water. Dissolve 2½lb. sodium carbonate in another vessel of water. Mix the two together, and before using add 1½ pints of ammonia, and then dilute to 30 gallons with water.

The Secretary read the following correspondence:—

151, Macquarie-street, May 12th, 1891.

"Dear Mr. Morton,—I regret I shall be unable to be at the Royal Society's meeting to-night. Would you mind recording the existence in Tasmania of the two following plants:—

Lobelia rhombifolia, found in October, 1890, on the road from St. Helens to Constable Creek by Wm. Fitzgerald. The plant was sent to me, and I forwarded it to Baron Von Mueller, and I enclose his reply. Unfortunately I have no specimens.

Orthoceras strictum, found in December, 1890, by Mr. Geo. Hinsby near George's Bay. I recognised the plant as *O. strictum*, and when Baron Von Müeller was here he confirmed it I leave the dried specimen for your examination.

Kindly let me have the letter and specimen again, as I have to return the former, and wish to keep the latter.

Yours faithfully,
L. Rodway."

"Let me congratulate you, dear Mr. Fitzgerald, for your discovery of a plant, new as regards the records of the Tasmanian flora. It is the *Lobelia rhombifolia* of De Viese, originally found by Drummond in West Australia; in 1848 traced by me into South Australia, and since then found in some few places of Victoria. The Tasmanian locality will be recorded in my work under your honoured name. Where this plant occurs perhaps you may find other rare plants yet. As you seem to take an interest in the indigenous plants of your beautiful island I would particularly remark that the floating and submerged plants there require still more attention there than any others, even as far as the small water lentils. It would be well in each instance if the specimens were pressed and dried before they are sent off, as otherwise they suffer in transit. May I advise you to send to Mr. Alex. Morton, Secretary of the Royal Society of Tasmania, a note regarding the discovery of the Lobelia in your Island, and to dry specimens for being submitted to the next meeting in Hobart.

Regardfully yours,

FERD. VON MUELLER."

CALCULATING MACHINE.

Mr. R. M. JOHNSTON gave a lucid and practical demonstration of the method of working a new calculating machine introduced into the Statistician's Department to facilitate the work of that department in calculations. The machine is called the "Arithmometer." In the absence of the author Mr. Johnston read a paper, prepared by Mr. F. Jacobs, A.I.A., entitled "Notes on the Arithmometer as an aid to actuarial work."

The Secretary, in the absence of Mr. A. B. Biggs, read a paper prepared by that gentleman entitled "The Possibilities of the Telescope."

Mr. A. Montgomery, M.A., Government Geologist; Mr. W. F. Ward, Government Analyst, and Mr. T. Stephens, M.A., and Mr. R. M. Johnston, replied at some length to the paper of Mr. Powers, read at the last meeting, entitled "Notes on the Mt. Lyell District, Tasmania.

On the motion of His EXCELLENCY, the customary vote of thanks was passed to those who had contributed papers and remarks, and the proceedings terminated.

JUNE.

The monthly meeting of this Society was held on June 9, the President, His Excellency Sir R. G. C. Hamilton, K.C.B , presiding.

PAPERS AND PROCEEDINGS.

The SECRETARY laid on the table the published papers and proceedings of the Society for the year 1890.

ANTARCTIC EXPEDITION.

His EXCELLENCY said that at the last meeting of the Royal Society a resolution was passed asking the Council to wait on Ministers to ascertain whether they would be prepared to recommend Parliament to vote a certain sum towards the expenses of the Antarctic expedition under Baron Nordenskiold. Before, however, any action was taken by the Council, an intimation was received from Ministers that they had determined to ask Parliament for £300 towards the proposed expedition. He telegraphed that fact to Baron Von Mueller, and received the following telegram in reply :—" Grateful for support Tasmanian Government to Nordenskiold expedition. This greatly strengthens the whole cause." He did not know how far the matter had progressed since, but thought it satisfactory that in Tasmania everything had been done that could be expected in this matter.

AUSTRALASIAN ASSOCIATION FOR ADVANCEMENT OF SCIENCE.

His EXCELLENCY thought it desirable that a progress report should be made by Mr. Morton in reference to the arrangements for the approaching meeting of the Australasian Association for the Advancement of Science.

Mr. MORTON said that he had sent out 950 circulars to members of the Association and others throughout Australia requesting them to notify whether they intended to be present, although he had not received one quarter of the replies expected, already 150 had notified their intention of being present at the meeting to be held in Hobart on January 7 next. Papers had been promised by able contributors in the following sections :—Astronomy, chemistry, geology, biology, geography, economic and social science, anthropology, literature, and fine arts. He regretted to intimate that Sir Wm. McGregor, Governor of British New Guinea, had written stating that it was impossible for him to leave his work there early next year in order to attend and occupy the position of president of the geographical section. Mr. Morton further stated that liberal concessions in the matter of travelling rates had been granted to members, and he had no doubt that the result would be a large influx of visitors. At the meetings of the Association in New Zealand last year, large numbers of local residents became members of the Association, and he hoped a similar result would take place here, as by becoming members they would have the privilege of attending the different sections.

His EXCELLENCY said : I think we may be satisfied with the progress made as detailed by Mr. Morton. I may say that I also had a letter from Sir William McGregor similar to that received by Mr. Morton, but I thought the matter of so great importance, that I wrote to him again. I am sure it will be very agreeable to this Society to learn that I have succeeded in getting Mr. Giffen, the eminent statistician and political economist, to agree to pay me a visit during the session of the Australasian Association for the Advancement of Science to be held here in January next, and to read a paper before the Association. Mr. Giffen, as many of you know, is one of the highest living authorities on economic science, and I am sure that his presence on this occasion will be greatly appreciated by the members of the Association, and that it will give an impetus to the study of this science, which will bear fruit throughout all the Australasian colonies. (Applause.)

The Secretary apologised for the absence of Mr. R. M. Johnston, owing to indisposition, and read the following remarks, prepared by that gentleman in connection with this subject:—It is pleasing to observe with what enthusiasm His Excellency Sir Robert Hamilton, the president-elect of the Australasian Association for the Advancement of Science, is endeavouring to make this, the fourth meeting, taking place in Hobart in January next, one of the most successful of the series yet held. The Association, mainly owing to the instrumentality of its present permanent general secretary, Prof. Liversidge, was founded in the year 1886, and has since held three very successful meetings; the first in Sydney, in January, 1888; the second in Melbourne, in January, 1890; the third in Christchurch, New Zealand, in January of the present year. The Association follows exactly the lines of the British Association, and like that Association its objects are—To give a stronger impulse and a more systematic direction to scientific enquiry; to promote the intercourse of those who cultivate science in different parts of the Australasian colonies with one another and with foreign philosophers; to obtain a more general attention to the objects of science and a removal of any disadvantages of a public kind which may impede its progress. Science in Australasia has already received a great impulse from these yearly gatherings, the direct and indirect results of which will have a most important bearing upon the intellectual, social, and material progress of these colonies. It is evident from what has been disclosed at the last meeting of the Royal Society that His Excellency Sir Robert Hamilton does not intend that one of the express objects of the Association should remain a barren phrase, viz.:—"To promote the intercourse of those who cultivate science in different parts of the Australasian colonies with one another and with foreign philosophers." Already he has, through the energetic General Secretary (Mr. A. Morton, F.L.S.), received the assurance that at its next meeting in Hobart will be assembled one of the largest gatherings of scientific men of Australasia which has yet been brought together, including nearly all the most prominent names in various branches of science, art, and philosophy; and he has also endeavoured to throw additional lustre upon this gathering by cordially inviting as his guests for the occasion several of the most illustrious men of science in the Mother Country. One of these, Professor Huxley, has written a most cordial letter regretting that the state of his health alone prevents him from accepting His Excellency's hospitality and denying himself the pleasure it would afford him to re-visit Tasmania, of which he has still many pleasant recollections. It is gratifying, however, to know that he has received the assurance that Dr. Giffen, LL.D., now created a C.B., one of the most illustrious economists and statisticians of Europe, is to become his guest, and will be present to take part in the work of the Association. In economic science the name of Dr. Robert Giffen stands in the very first rank of the most illustrious names in Europe, and there is no authority in Europe who at the present moment is held in greater respect, and whose writings exert a deeper and wider influence on the great political and social economic problems which at present disturb the industrial relations of nearly all civilised countries.

Of the many distinguished Presidents of the Royal Statistical Society of England it is certain that he ranks among the foremost, while his numerous contributions to the journal of that Society, in connection with important questions of the day, reveal what a marvellously wide and thorough grasp he possesses of the facts which bear upon the subjects which he discusses, and of the still more remarkable power displayed by him in the manner in which he brings his facts into logical array, and in drawing important conclusions therefrom. Perhaps in this respect he has no living equal in his own branch of science. The following is a list of the more important papers on statistical and economic subjects contributed by Dr. Giffen to the Royal Statistical Society since the year 1879:—(1) "On the fall in prices in recent years."—(Journ. S.S., Mar. 1879). (2) "The progress of the working classes in the last half century (presidential address).—(Journ. S.S., Dec. 1883.) (3) "Further Notes on the progress of the working classes in the last half century."—(Journ. S.S., Mar. 1886). (4) "Recent rate of material progress in England" (presidential address, Brit. Assoc. 1887).—(Journ. S.S., Dec. 1887). (5) "Recent changes in prices and incomes compared."—(Journ. S.S., Dec. 1888.) (6) "Accumulation of capital in the United Kingdom in 1875-85."—(Journ. S.S., Mar. 1890). Dr. Giffen's presidential address to the Royal Statistical Society in 1883, "The progress of the working classes in the last half-century" was a most remarkable one, and created a profound impression throughout Europe, America, and these colonies, and the effect on the minds of the leading statesmen of the United Kingdom is best indicated by the fact that at Mr. Gladstone's special request the address was reprinted, and widely circulated throughout the United Kingdom. Dr. Giffen is also widely known as the author of several important works on financial matters. The presence of such an illustrious economist will add great lustre to the coming meeting of the Association, and will be especially welcomed by Section F.—Economic and Social Science and Statistics—where his active support and influence will undoubtedly increase the interest and usefulness of its proceedings.

SOUTHPORT CAVES.

His EXCELLENCY said that there had recently appeared in the Press an account of some caves recently discovered at Southport. Mr. Morton formed one of the party who visited the caves, and brought away some specimens, and he thought it would be interesting to hear from him an account of that visit.

Mr. MORTON said that an opportunity having been afforded to visit the caves in the vicinity of Ida Bay, near Southport, he took advantage of it, and notwithstanding the short time at his disposal he thought that what he saw was sufficient to justify writing to the Minister of Lands, drawing his attention to the very urgent necessity of having these caves thoroughly examined and a report furnished, with a view of having them protected, under similar conditions to the well-known Jenolan and Fish River Caves in New South Wales. As these caves were of easy access from Hobart, excursionists being able to reach them within five or six hours, they should undoubtedly prove a favourite resort to tourists and visitors to the colony. They were situated about four miles from Ida Bay, and a fairly good road led to them. On reaching the mouth of the caves it was seen that the entrance was through a limestone formation. There was a very strong stream flowing along the floor of the cave. The first chamber reached showed some fine stalactites, and along the floor some fine stalagmites were seen. On extinguishing the lights carried by the party the ceiling and sides of the caves seemed studded with diamonds, millions of glow-worms hanging to the sides of the walls and from the ceilings causing this phenomenon. Proceeding further, several chambers were explored, each revealing grander sights. The time at disposal being limited, the party had to return after traversing a distance of about three-quarters of a mile, but from what was observed the caves evidently extended a distance of three or four miles. The only life met with in the caves were the glow worms previously mentioned. These caves, under proper supervision, should become one of the great attractions of the South.

Mr. BERNARD SHAW concurred in the recommendation made by Mr. Morton, and hoped it would be adopted by the Minister of Lands. He thought that there would be very little trouble in having those caves protected, as there was a station with resident police at Southport.

NEW ZEALAND EXHIBITS.

Mr. MORTON, as Curator of the Museum, drew attention to specimens of greenstone from New Zealand, also of axes and knives used by the Maoris, the latter being made out of obsidian, the gift of Messrs. A. Hamilton and F. R. Chapman, of Dunedin, New Zealand, to the Tasmanian Museum.

The Secretary read, in the absence of the author, the following papers :—1. " Results of observations of the transit of Mercury on May 10, 1891." 2. "The total eclipse of the moon on May 24, 1891." By A. B. Biggs.

JULY.

No meeting held.

AUGUST.

The monthly meeting of this Society was held on Monday evening, August 17. The President, His Excellency Sir R. G. C. Hamilton, K.C.B., presided, there was a large attendance of Fellows, also a number of ladies.

NEW MEMBERS.

Dr. O. Burchard, of Hamburg, was elected a corresponding member, and the following to be members :—Walter Spencer, M.D., Launceston; W. F. M. Shells, M.R.C.S.E.; George Henry Gibson, M.B. and C.M., Edinburgh; Wm. Earle, Mount Zeehan; Ernest Milton Law, and Wm. Dubrelle Weston.

MACQUARIE ISLAND.

The Secretary read the following letter from Captain Fairchild, of the New Zealand Government steamer Hinemoa.—"Wellington, June 3, 1891 :—I see your Government has prohibited sealing on the Macquarie Island. It is a wise step, because if it had gone on a few years longer there would not have been a seal or a sea elephant left on the island. I think the penguins (the king penguin *Aptenodytes longirostris*) should be protected for, say, five years on Macquarie Island, or else they will soon be all gone—that beautiful bird is only found on the Macquarie Island. There are none on any of the islands belonging to New Zealand, and as they are such a helpless bird and so easily caught, there will not be one left in five years if they go on killing as they have the last year or two. So I hope soon to hear that the king penguin are not allowed to be killed on Macquarie Island for the sake of their oil. The man on the island told me he killed 15,000 last year for their oil, from which he got 15 tuns of oil."

Sir LAMBERT DOBSON suggested that as the subject was a new one it should be considered by the Council and afterwards by the Society, so that it could go before the Government after mature consideration. He moved,—"That the letter be referred to the Council of the Society," which was agreed to.

SCIENTIFIC INFORMATION.

In the absence of the author, Mr. A. J. Ogilvy (the SECRETARY) Mr. Morton read an abstract of the paper prepared by that gentleman, entitled "Suggestions as to the best means for collecting scientific information." The writer suggested that the Royal Society should obtain the services of some resident in each district of the colony, if possible, to act as its hon. representative in that district. He should commence as soon as he conveniently could to collect specimens illustrating the geological features and useful products of his district as, generally, of the granite or other chief rocks, especially of the coal, limestone, fireclay, or other useful products, for comparison with similar specimens from other districts. Also any striking examples of petrifaction, crystallisation, etc., in short anything likely to be of scientific interest. On hearing of any interesting discovery in his district, he would put himself in communication with the discoverer and report to the Society. He would be the recognised agent of the Society in the district to whom discoverers and inquirers would voluntarily apply to give or receive information; to whom also the Society could refer for any special local information it might require. Such an appointment would not only serve many direct practical ends but would also arouse increased interest among these co-operators in scientific subjects and in the affairs of the Society; would stimulate them to closer attention to their surroundings, and would help to propagate an interest in scientific progress throughout the colony.

AUSTRALASIAN SCIENTIFIC ASSOCIATION.

His EXCELLENCY asked if there was any progress report *re* the meeting of the Australasian Scientific Association for the Advancement of Science in January next.

Mr. MORTON said that so far everything was progressing very favourably. Some 200 members had already signified their intention of being present, and a number of papers had been promised. He had also pleasure in intimating that ladies would be admitted to the meetings and have the privilege of taking part in the discussions and reading papers on payment of a small fee.

PAPERS.

TASMANIAN INSECTS.

The SECRETARY read an interesting extract from a letter written by Mr. J. J. Walker, R.N., Chief Engineer of H.M.S. Penguin, in which he stated that the collection of insects made by him during his visit to Hobart had turned out well. He had sent to the British Museum 1,122 mounted specimens. They consisted of 586 species of coleoptera from Hobart, and 1,871 specimens of 798 species from all Tasmania, being perhaps one-fourth of the varieties of insects in the island.

HYPNOTISM AND ALLIED PHENOMENA.

Dr. WALTER SPENCER, Launceston, read a paper on the above subject. The paper was a lengthy and elaborate one, and in it Dr. Spencer dealt with the established facts in regard to Hypnotism, and described the sensation which it caused among members of the medical profession when introduced as a means of painless operations. Although its use in the alleviation of pain among the medical profession in England gave place to chloroform and ether it was taken up in France where two different schools prosecuted its study. He gave illustrations of the rival methods employed by practitioners of this new power, and pointed out the dangers of its practice, which caused the British Medical Association to pass a special resolution on the subject. He described how he had practically studied the subject in London 15 years ago, and at the end of a series of 12 lessons was able to relieve small pains by its use. He showed the identity of hypnotism with mesmerism, and gave a most interesting account of the history of such phenomena illustrated by numerous examples. A discussion followed, in course of which Messrs. A. J. Taylor, R. M. Johnston, Justice Adams, and Sir Lambert Dobson took part, after which a vote of thanks was awarded to Dr. Spencer.

His Excellency retired to fulfil another engagement, and His Honor Sir Lambert Dobson took the chair.

TASMANIAN PLANTS.

The SECRETARY read some brief remarks by Baron von Müeller. bringing under notice some rare Tasmanian plants lately found by Mr. T. B. Moore on the highlands east of Mount Tyndall. Baron Müeller considered the plants to be a new species of *Coprosma*, its characteristics being a very depressed, matted growth, very small leaves, and terminal small-sized fruit. The leaves were pointed, and the fruit beautifully blue outside. He recommended that if the plant proved a peculiar species it be distinguished under the finder's name.

Baron Müeller also acknowledged indebtedness to Mr. Moore for providing him with specimens of the *Panax Gunnii*, a rare shrub, found in deep shady gorges at Mount Lyell on the Canyon River, the Franklin River, and on a tributary of the Pieman River. From the highlands of Mount Read and Mount Tyndall, Mr. Moore sent specimens of the *Pentachondra verticillata*, a species of *Styphelia*, which until now was only known from Dr. Milligan's collection.

MOUNT LYELL.

The SECRETARY, in the absence of the author, read notes on the Mount Lyell district by Mr. F. Danvers-Power, F.G.S., being a rejoinder to criticisms by members of the Society on the paper recently written by him on that subject. The writer expressed surprise that his critics credited him with being the author of new theories. It was possible that he was the first to apply the views expressed in his paper to Tasmanian geology, but he did not pretend to be the originator of what Mr. Stephens termed the "water pressure and wave theory," or of the anti-true-fissure-lode theory, although he was prepared to uphold them until disproved, or more suitable substitutes were brought forward, His paper was intended to give but a sketch of the geology of the Mount Lyell district. He was aware from Tasmanian literature, as also by personal conversation, that his views were in opposition to those generally accepted in the island, but by bringing the results of modern research to bear on local conditions, he had hoped to direct thought out of the groove into which it had fallen. Of course he did not expect to win those who had strong views on the subject over to his side at once, but he did expect them either to give valid arguments for their side of the question, or to prove his incorrect, especially when those sentiments were due to misconceptions on their part. He then dealt at length with the points on which his critics differed from him—(1) the main cause of mountain ranges; (2) the nature of most so-called true fissure lodes.

Discussion on Mr. Power's paper was postponed to a future meeting.

This concluded the meeting.

LIST OF ADDITIONS TO THE LIBRARY.

Aborigines of Australia. A paper read before the American Antiquarian Society, Boston, April 30, 1890, by E. E Porter, M.A. From the author.

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Bulletin of the United States Geological Survey :—No. 58. The glacial boundary in Western Pennsylvania, Ohio, Kentucky, Indiana, and Illinois. No. 59. The Gabbros and associated rocks in Delaware. No. 60. Report of work done in the division of Chemistry and Physics mainly during the fiscal year 1887-8. No. 61. Contributions to the Mineralogy of the Pacific Coast. No. 63. A Bibliography of Palæozoic Crustacea from 18 to 1884. No. 64. A report of work done in the division of Chemistry and Physics mainly during the fiscal years 1888-9. No. 66. On a group of volcanic rocks from the Tewan Mountains, New Mexico, and on the occurrence of primary quartz in certain basalts. From the Department.

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Catalogue de la Bibliothique, Fondation Teyler. Harlem. From the Department.

Catalogue of the Chinese Imperial Maritime Customs Collection, at the U.S. International Exhibition of Philadelphia, 1876. From the Department.

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Catalogue of the fossil Cephalopoda in the British Museum Natural History, Pt. II., containing the remainder of the sub-order Nautiloidea, consisting of the family Lituitidæ and Nautilidæ, with a supplement by A. H. Foord, F.G.S. Catalogue of the fossil Fishes in the British Museum, Pt. II., containing the Elasmobranchii, Acanthodii, Holocephali, Icthyodorulites, Ostracodermi, Dipnoi, and Teleostomi, and Chodrostean Actinopterygii, by S. Woodward, F.G.S.,F.Z.S., etc. From the Trustees.

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SEPTEMBER.

The monthly meeting of this Society was held on Tuesday, September 8, the President, His Excellency Sir R. G. C. Hamilton, K.C B., in the chair. There was a large attendance of members and ladies.

The following were elected Fellows of the Royal Society :—Ven. Archdeacon Mason, Rev. James Scott, D.D.

His Excellency referred to the paper read on behalf of Mr. A. J. Ogilvy at last meeting on the best means of collecting scientific information, and suggested that the matter should be brought under the notice of the approaching meeting of the Australasian Association for the Advancement of Science. He considered that valuable information would be obtained if, as suggested, there was a representative of the Society in every district to keep his eyes and ears open, and let them know of anything of scientific interest.

Mr. A. J. Ogilvy said he regarded it as very important that in every district the Society should have some one to represent it, and pointed out that if at any time it wanted any special local knowledge there would be somebody to whom application could be made. He thought that the Fellows should consider as to the best means of carrying out the suggestion made, and deal with the subject at a future meeting.

Mr. A. Morton, as General Secretary to the Association, gave a satisfactory progress report *re* the arrangements for the approaching meeting of the Australasian Association for the Advancement of Science, and hoped at next meeting to present a report giving a list of the papers to be read.

ELECTRIC TRACTION.

Mr. Montague Jones, C.E., read a paper on the above subject. He said that whether the present generation had discovered the ultimate force in nature most applicable to the service of men was a question for the scientist of the future to decide, but he thought they could claim the present to be an electrical age, as in pre-historic times there were ages of stone and bronze. The practical utility of the application of electricity to the propulsion of railway and tramway cars was first demonstrated at Berlin in 1879 by Siemens and Halske on an experimental line of 500 metres, in the form of an oval. The train consisted of a small electric locomotive and carriages, which had very small wheels, with two rows of seats running parallel to the rails. The success of this experiment led to other attempts of an exhibitional nature at Brussels, Dusseldorf, Frankfort and other places, and then electrical traction passed from the experimental to the commercial process of development. The Lichterfelde electric tram near Berlin was the first of its kind. The length was 1½ miles, and the equipment in 1881 consisted of two motor cars, the motion being transmitted to the wheels by belts working on grooved pulleys outside the wheels. The prime source of power was a steam engine, with a Siemens motor and generator, but the installation differed in some respects from the Berlin line, the central rail not being used, but the one rail acting as a lead, and the other as a return for the current. Up to 1887 this line carried 100,000 passengers yearly. These instances showed that the inception of electrical tramways took place in Europe, the principle being the generation of electricity by dynamo, and conveying the current through conductors connected by sliding contact with the cars while in motion. Modern electrical railways were now built chiefly on this principle, although America had far succeeded Europe in the improvement and perfecting of the system. The remainder of the paper consisted of a description of the various systems in vogue, after which he dealt with the "overhead system," which had most successfully operated commercially and practically. Many objections of an æsthetical nature had been urged against the overhead gearing, but they were more apparent than real, and he recommended such objectors to look to the telegraph cables so obtrusively conspicuous in big towns. It was superior to other systems of traction. Traction expenses on horse tramways amount to two-thirds of entire working expenses, to say nothing of its inhumanity, and steam was a clumsy, though effective, method of applying force. He considered that the citizens of Hobart would be wanting in public spirit if they permitted steam engines to run along their streets. If a draughtsman sat down and deliberately attempted to design an affront to a decent community, he could not have succeeded better than at Sydney, where the working expenses amounted to three shillings per tram mile. It was now generally admitted that the struggle for existence lay between the cable system and the electric. The cost of the construction of the former in Melbourne amounted to £34,000 per mile and 75 per cent. of the available energy was lost by dragging the cable itself. At Minneaopolis, £80,000 was spent in the purchase of cable plant which was cast on one side as scrap iron to make way for an electrical system. The cable tramway could certainly surmount phenomenal grades, but on the other hand the electrical tramway could negotiate 1 in 8 grades, or even less. It was only a question of power, and Mr. Reis, an electrical engineer, had made some very valuable discoveries as to electrical braking and adhesion, which he hoped to see practically demonstrated at an early date. It was difficult to surmise what shape electrical developments would take place in future, its potentiality being apparently infinite, but it was no stretch of imagination to say that it was the locomotive power of the future. The various stages in the history of electric traction as described in the paper were illustrated by diagrams shown with limelight effects by Mr. Nat Oldham, and assisted greatly in conveying a clear comprehension of the lecture to the minds of the audience.

Mr. JOHN MACFARLANE said he had an opportunity of gathering information on this interesting subject, being in correspondence with the Thomson-Houston Company, makers of electric railway plant. Electric traction had left the region of scientific experiment and was an established commercial success. In the United States there were 310 tramways or railroads worked by electricity, with 4,000 cars and 7,000 motors. It was estimated that one-third of the street railway mileage in the States was worked by electric traction, and he referred to a number of companies which paid 8 per cent. With regard to steep grades he believed that 1 in 7 was not too steep for working, but he believed that the steepest grade in the proposed Hobart line was 1 in 16. It was interesting to note further that electric traction had also been successfully applied to tramways for mills and manufactories. In conclusion, he referred to the testimony as to the safety of the overhead wire system.

Mr. C. W. S. JAMES said he had been deeply interested in the paper read, and had tried to think how far the electric traction system could be applied to Hobart. A few months ago he had estimated what could be done in Launceston by utilising the South Esk River and he thought that there from 1,000 to 1,300 horse-power could be available. In regard to Hobart, however, there was not the same power available, and he believed they would require to look to generated power in any electrical system introduced. He thought the system could be usefully applied to carrying minerals on the West Coast, and that the "overhead system" was well adapted for the streets of Hobart.

Mr. J. Fincham said he had followed the paper with interest, but they had no reference to the length of the steep grades surmounted. He believed that in any scheme for electrical traction in Hobart this would be one of the difficulties. He was aware that there were about 300 tramways in America, but he had not been able to get any particulars as to the grade, or whether it was equal to such a grade as they had to contend with in going up Elizabeth-street.

Mr. A. W. Lawder thought the length of grade to be overcome simply resolved itself into a question of the power of the accumulator.

Mr. Fincham said that was just the difficulty the engineers found. They had not been able to provide enough accumulation to overcome a sustained strain.

His Excellency, in bringing the proceedings to a close, thanked the author of the paper, and all who had taken part in the discussion. He was pleased to see such a large meeting, and was quite sure that Mr. Jones, by his interesting paper, had justified the expectations of all present.

The proceedings then terminated.

OCTOBER.

The monthly meeting of the Royal Society of Tasmania was held on Monday evening, October 12th, the President (His Excellency Sir Robert G. C. Hamilton, K.C.B.,) in the chair, a large number of ladies and members being present.

The President said before calling upon the Secretary to give a report on the progress of the Australasian Association for the Advancement of Science he was sorry to have to inform the members that Sir Wm. MacGregor had wired to him stating that he would be unable to be present at the meeting to be held in January. His Excellency said he was pleased to say that he had a letter from his friend Sir Robt. Ball, Astronomer Royal of Ireland, who, although unable to be present, had promised to forward a paper to the astronomical section. (Hear, hear).

The Secretary (Mr. Morton) said Professor T. W. E. David, F.G.S., Professor of Geology at the Sydney University, had been asked and had accepted the presidency of the geological section, rendered vacant by the death of Mr. C. S. Wilkinson, and for the geographical section Commander Pasco, R.N., of Victoria, had been appointed. Committees of the various sections had been appointed, and the preliminary programme was now in the hands of the printer, and would be ready in good time. Titles of papers had been received from members residing in the several colonies, and the bulk of most of the papers no doubt would be forwarded during the next few weeks. Mr. Morton referred to the recent meeting of the American Association that had met in the United State and said he hoped that, as at that meeting, at the Tasmanian gathering many ladies would take part, not only in the discussion, but would contribute papers to the several sections. (Hear, hear.) The secretary further remarked that he was pleased to say everything in connection with the coming meeting promised well.

His Excellency said before calling upon Mr. Johnston to read his paper, he would like to say that at the last meeting Mr. Montague Jones had, owing to some oversight, been unable to reply to those who took part in the discussion following Mr. Jones' paper, and he would now ask Mr. Jones to reply.

Mr. Jones said in a discussion which followed the reading of the paper on electric traction Mr. Fincham appeared to associate the "storage system" with the practical operation of electric trams, but it was clearly pointed out that the "storage" up to the present time was an ideal method, and that the "overhead system" was the only one of practical value. The main defects of the former are the limited capacity of the cells, and when brought to bear on a grade of 5 per cent. or over heat instead of current was developed, which buckled the cell plates, rendering them useless, and again, the cells are composed of lead, weighing altogether 3,000lb., which has to be carried about over light as well as the heavy grades, losing 60 per cent of the available efficiency. Coming to the "overhead system," the question of surmounting heavy and continuous grades resolves itself into a question of increased power, which is always to be obtained from the central station, and herein lies the great advantage the overhead system has over any other. All the available energy can be utilised excepting a few points per cent., due to the resistance of the current by the conductor. The maximum grade in Hobart is 1 in 16, and is slight compared with some of the towns where cars are being operated by electricity. The following is an extract from the directors of the "Richmond Union Passenger Line" to Mr. Sprague :—" The road which you have equipped under most trying conditions has been one of the most, if not the most, difficult which could be met with in street railway work. The excessive and continuous grades, the numerous sharp curves, the gradients in these curves, the weight of the cars, and the heavy roads which they have been required to carry together with the extent of the system, and the number of the cars in operation (about 40) constitute the enterprise, the largest and most difficult yet inaugurated in any part of the world. We acknowledge the successful fufilment of all the terms and conditions of the contract, and compliment you upon having achieved so signal a success." The number of street electric railways at work, and the number contemplated, is quite sufficient to prove that it has long since passed beyond the experimental stage of development. Citizens of Hobart should keep these facts in view and refuse to listen to any argument stating it to be impracticable to introduce an electric service here; the wretched apology for a permanent way will certainly have to be removed, and a neat steel grooved rail put in its place, as the basis of efficient street transit is a smooth and sound rolling surface. I would also like to see a 4ft. 8½in. gauge, instead of a 3ft. 6in. gauge, as the latter might create mechanical difficulties which cannot at present be foreseen, as all the electric roads, or nearly so, are built to narrow gauge requirements. Of course these difficulties would be overcome; still it is just as well to be on the safe side. As regards a prime source of power for Hobart, it is a simple question of cost and maintenance whether water or steam would be better. There is plenty of water, and the cost to supply a constant head would be, I think, much less than coal. Two hundred h.p. would be more than sufficient force for a service of 12 cars, and as to the mechanical power for equipment it does not make much difference whether the grades be 1 in 5 or 1 in 10, as the motors used are of a standard size, having a capacity of 15 h.p. each. Where the grades do not exceed 4 to 5 per cent., one motor could do the work, but where the grades exceed 5 and run up as high as 10 per cent., then it is unsafe to operate, except by driving both axles, and then the equipment must be two 15 h.p. motors. The average weight for street motors' equipment is about 100lb. per h.p. Hence 2·15 h.p. motor equipment will weigh about 3,000lb. I have deduced this to mathematical formulæ following:—H.P. 4·75=M.T, (C x 1), where M miles per hour, T No. of tons, C rise in feet per 100. The cost of single line "overhead" construction, including permanent way material, under average conditions should not amount to more than £3,500 per mile. This, in comparison with the cable system at Melbourne at £34,000 per mile is very marked, and the economy of the overhead system is so manifest that Mr. Henry Peabody, of Boston, wrote to me as follows, in reply to inquiries:—"There is a feeling among all Municipal Councils, where railways apply for 'overhead lines,' that the increased economy warrants their asking for a decrease of fares, hence their desire to keep quiet about their balance-sheets." Mr. James asked me to explain the Telpherage system. There are two, "the series" and the "cross ones parallel." The latter is now being operated in many places. The skips or trucks are suspended from an iron rod supported by poles, and the rod acting as a conductor, is constructed on the "break and make" principle at every 120ft. or so, taking its supply of current from a dynamo fixed at a convenient place. The "makes and breaks" are normally closed, so that a current of electricity may flow from end to end, but when the first wheel of the skip of a train touches the "break" the circuit is closed and the currentruns back to the last wheel of the train, and into the skip containing the motor and thereby energising the train. The same operation to continues at the intervals stated the end of the journey. It can be worked up to 15 miles per hour with ease, and the cost of carrying is about 1s. 2d. per ton per mile. Unlike most new inventions "Telpherage" does not persist in adhering to any principle of an obsolete type, but is an innovation so extraordinary as to pass all practical experience. As feeders to the West Coast lines nothing could be more admirable, and not as the remote future we shall see it universally adopted, instead of those useless unpayable lines and costly roads that cripple the revenue of new countries.

Mr. W. F. WARD (Government Analyst), said he did not intend to say very much, as he had expressed himself at a previous meeting. He would, however, say that he entirely disagreed with the views put forth by Mr. Power.

Mr. ALFRED J. TAYLOR, F.L.S., read the following paper, entitled "Notes on the Shell-mounds at Seaford, Little Swanport."—Among the many interesting relics of the aborigines of Tasmania that yet remain, not the least interesting are the shell-mounds that mark the spot where they formed their encampments and feasted before the intrusion of a white race had disturbed their simple and peaceful modes of living. Such shell heaps as that to which I am about to refer occur in other parts of the world, and for some time they were supposed to be nothing more particular than ordinary "raised beaches." Even now in Tasmania there are many well-informed people who cannot bring themselves to believe that the shell heaps noticeable in many places on our shores are the kitchen-middens, or "refuse heaps," of the aborigines of the colony; and it is for the information of such sceptics that I place on record the following evidence as establishing beyond all reasonable doubt the fact that they are this, and nothing more. One has only to examine these remarkable accumulations to obtain evidence of an intensely interesting and convincing character, and the results of a personal examination of the extensive shell mounds to be found on the estate leased by Mr. Samuel Drake at Little Swanport have induced me to hope that a few notes on the subject may be of some interest to the fellows of this Society.

Mr. R. M. JOHNSTON said he was very pleased to find Mr. Taylor had taken so much trouble in obtaining such valuable information dealing with the aborigines of Tasmania. The paper read by Mr. Taylor was extremely interesting from an ethnological point. He (Mr. Johnston) had paid considerable study to the working out of the shell deposits, and on a recent visit to the place referred to in Mr. Taylor's paper he had observed the remains of charred wood, proving that the great deposits of shells had been made by natives, and were not formed by other agencies.

Mr. E. D. SWAN said some years back Mr. Mitchell, who resided on the East Coast, had, at a meeting of the Society, spoken on this matter, and had been told by the natives themselves that they had de posited the oyster shells that formed the extensive deposits.

Mr. MORTON said the paper read by Mr. Taylor was a most important one from an ethnological point, the specimens collected being exceedingly interesting. Although there was a difference of opinion still existing regarding these shell deposits, from what he himself saw he could arrive at no other conclusion than they at one time were formed by natives. Although Darwin in his work "on the structure and destruction of coral reefs and geological observations on the volcanic islands," page 258, states that during his visit to Tasmania he examined several of the shell deposits, and was of opinion that while some of the mounds may have been formed by the aborigines, the greater number of these deposits he considered had been caused by a small elevation of the land. Captain Grey and other travellers have found in Southern Australia upraised shells, belonging either to the recent or to a late tertiary period. The Rev. W. B. Clarke found proofs of the elevation of the land, to the amount of 400ft. at the Cape of Good Hope. Darwin says that when visiting Tasmania he was assured by an intelligent farmer that oysters were formerly abundant in Ralph's Bay, but that in the year 1834 they had without any cause disappeared. In the "Transactions of the Maryland Academy," Vol. 1, Pt. 1, p. 28, there is an account by Mr. Ducatel of vast beds of oysters and clams having been destroyed by the gradual filling up of the shallow lagoons and channels on the shores of the United States. Darwin, when visiting New Zealand, says he observed in the neighbourhood of the Bay of Islands, that the shores were scattered to some height, as at Van Diemen's Land, with sea shells which the colonists attributed to the natives. He (himself) was of opinion that the land had been elevated. The bone Mr. Taylor had been fortunate to obtain from the shell deposits clearly showed that it had been made by the natives, and was a most valuable relic.

Mr. A. J. OGILVY drew attention to some caves at Rocky Mouth that he had visited, and had been dwelling places of natives. On examination he felt sure many interesting relics might be found.

Mr. TAYLOR, in reply, said he felt very pleased at the great interest his paper had caused, and promised to lay before the Society any further information he might obtain, and also he would be pleased to present to the Museum any specimens he might have in duplicate.

Mr. W. A. WEYMOUTH exhibited a magnificent collection of Tasmanian mosses collected and mounted by him. The collection consisted of many species, Mr. Weymouth kindly promising to present a mounted set to the Museum.

Capt. SHORTT, R.N., also exhibited a choice collection of photographs of the "milky way and nubecutœ," taken at the Sydney Observatory, 1890.

THE EARTH'S CRUST.

Mr. R. M. JOHNSTON, F.L.S., read a paper entitled "Observation on the Causes of Elevation and Subsidence of the Earth's Crust." He referred to the difficulties of dealing with a subject which necessarily rests so largely on arbitrary values for unknown data, and stated that as this was so it was obvious that any view he might tend to favour must in his own mind be attended with a large measure of doubt, and on some obscure points his judgment must be suspended entirely. As a novel theory, however, has recently been referred to by Mr. F. Danvers-Power relating to the elevation of mountain chains, he entered more fully into the consideration of those hypotheses of causation which have gained the most favour among physicists and geologists. He divided his paper under the following heads:—Formation of continental areas and ocean basins; contraction theory; the alleged expansion and contraction of the underlayers resulting from a rise or fall of temperature caused by the loading or unloading of the areas affected; expansion and contraction of the underlayers resulting from a rise or fall of temperature; Mr. Danvers-Power's reference to the supposed influence of the pressure of ocean waters upon the formation of parallel mountain chains; and concluded with the thought that good reasons had been given for rejecting the theory put forward by Mr. Danvers-Power.

His EXCELLENCY, in moving a vote of thanks to the authors of the papers, Messrs. Johnston and Taylor, said that although Mr. Ward had spoken strongly against the views expressed by Mr. Danvers-Power, in a paper contributed by that gentleman at a former meeting, the fact of having Mr. Power's paper had been the means of causing Mr. Johnston to read a most valuable paper. With regards to Mr. Taylor's paper he was of opinion that that gentleman deserved the thanks of the Society for the very valuable information he had obtained.

The meeting then closed.

NOVEMBER—NO MEETING.

SOME ACCOUNT OF THE MUTTON BIRDS, OR SOOTY PETRELS *(NECTRIS BREVICAUDUS)*, AS SEEN IN THEIR HOMES AMONG THE FURNEAUX ISLANDS, BASS STRAITS, TASMANIA, FROM NOTES TAKEN DURING A VISIT TO THE LOCALITY IN MARCH, 1891.

By H. H. Montgomery, D.D., Bishop of Tasmania.

My duties called me last March to undertake a trip to the Furneaux Islands. Before I started I was aware that one of the most interesting subjects for an ornithologist in that part of the colony would be the habits of the mutton bird. But the reality so far surpassed my expectations that I have ventured to lay before the Society some results of my observations. Mr. J. B. Walker has shown me allusions to the subject in old books of travel. Mr. Barnard has been good enough to furnish me with the second volume of the Tasmanian Journal, in which a short but excellent account is given of the habits of the sooty petrel, or mutton bird, by Mr. R. H. Davies. As the paper was written 45 years ago it may be worth while putting on record what I now propose to lay before you. These petrels choose islands where the soil is composed of a loose sand, covered in places by a bush with a blue flower called "barilla," where they congregate for the purpose of digging the holes in which they lay their eggs. I have heard of a rookery in Port Davey: and among the Hunters Islands they breed on Trefoil, Sheephead, Doughboys, and the Petrels. On the northern coast there used to be large numbers on Waterhouse Island, until the pigs kept by Mr. Barrett found them out, and destroyed such vast quantities that the birds deserted the place, and now there are just a few which are carefully preserved. In the Furneaux group they used to breed in much greater numbers than at present, and I venture to hope that the chief effect of this paper may be a timely movement by the Government to save from almost utter destruction an industry which adds distinctly to the wealth of the colony, and supplies a healthy article of food. At present the most important homes of these birds in the breeding season are Chappell Island (called usually "Hummocky," because of the hill in the centre), Little Dog and Big Dog Island, Green Island and Little Green Island, and Babel Island. These places are nothing more than low sandy spots, from 300 to 1,200 acres in extent, with hardly a tree (except on Big Dog Island), and covered with long yellow grass growing up to the waist. Most uninviting looking spots they appear to be. As you pass them in the day time you might fairly suppose that there was no living thing upon them except an occasional shag or a Pacific gull. I have thought that a sailor wrecked on one of these little wave-beaten spots might even starve if he slept soundly at night, ignorant that under his feet there lay concealed hundreds of thousands of birds, which, when eaten fresh, are among the most delicious of foods. About the middle of September the sooty petrels visit these breeding places, coming in at night and remaining some ten days, in order to dig their holes. They effect this with their feet, scattering the sand behind them in clouds. When this preliminary task is finished they leave their future nests for four or five weeks, and are not seen at all in the neighbourhood. In the middle of November they commence laying eggs. Mr. E. D. Atkinson has taken fresh eggs on November 21. Whether the males feed the mother birds during the period of incubation I do not know. But as the males return every evening it is probable that they do. Only one egg is laid by each female, and it is doubtful whether in the case of the egg being destroyed another is laid. I failed to enquire at what period the young bird is hatched out, but I gather that it must be at the commencement of January. On the 3rd of March I landed on Little Dog Island, and received my first introduction to these most interesting sea birds. At 6 p.m. not a mutton bird was in sight. Indeed, during the fortnight that I was cruising about in a boat I never saw a petrel in the day time anywhere. It is their habit to fly away to sea very early in the morning to distances 40 and 50 miles away, returning only when it is dusk. Whether they have long ago cleared the immediately surrounding waters of the food they require I cannot say; but it is a surprising fact that not one of the old birds is ever visible in the neighbourhood of their young throughout the day. Walking about on a rookery is a matter of no mean difficulty. The ground, always loose and powdery, is everywhere perforated with holes from two to three feet deep, about the diameter of a rabbit-burrow. Among the long grass it is almost impossible to prevent crushing down the sand and endangering the lives of the young birds by suffocation, at the same time falling headlong oneself. Just at sunset I was invited to go some two hundred yards up on to the higher ground—the island is only 200 acres in extent—in order to see the birds come in. I shall never forget that evening as long as I live. The sun was setting, leaving a broad belt of crimson on the western horizon, and soon the surrounding sea became almost invisible. Not a sound was heard except the rustling of the grass in the wind. There was no indication that there was a living thing on the island. There were no cries of sea birds. The stillness was wonderful. Presently a single dark-winged form flitted across the island and vanished again into the gloom. In another ten seconds thousands upon thousands of birds seemed to spring like magic up out of the darkness from every quarter without warning or cry of any kind. And now backwards and forwards before my dazzled sight I saw these countless dark shadows shooting with lightning rapidity athwart the last of the evening light. Still no articulate sound was heard. Nothing but the whistle as if of bullet after bullet through the air, bewildering one with the sense of numbers and of mysterious rushing life. Repeatedly a bird would dash within an inch of my head, and then wheel like lightning to one side to escape a collision. So imminent seemed the danger of arriving at home minus an ear or a nose from contact with a sharp beak that I was fain to crouch down in the long grass to escape an accident. To sit down on the ground on that particular island was possible because there were no snakes. Nothing would have induced me to have taken up the same position in the month of March had I been upon the island called "Hummocky." But of this hereafter. The minutes passed, and still this dizzy, whirling, hurly-burly of creatures continued—silent, and even awe-inspiring. Sometimes they came in squadrons of hundreds, sometimes by tens. But still they came; each bird after a turn or two sinking with unerring instinct on to its hole, finding it in the long grass and in the darkness with a certainty which was truly marvellous. It was difficult to tear oneself away from this wonderful spectacle. But at length we returned to our tent, pitched near the water's edge but still among the bushes, and all night long as I lay trying to sleep I heard the cooing and cackling of innumerable birds feeding their young in their subterranean homes, some of them apparently within a yard of my ear. At length I fell asleep, and before I awoke, at 6 o'clock in the morning, there was not a bird to be seen on the island. All the old petrels had long ago sped away to their distant feeding places. I regretted that I had not witnessed their morning exit. In its way it is as striking as their homeward journey. For, as these birds cannot fly off the ground, especially in the long grass, each one has to walk either to the sea shore or else to the top of some rock before it can take its flight. In some cases this journey must have meant a distance of many hundred yards. Mr. Davies relates in his paper, read in 1846, that the sealers showed him the manner in which they caught the old birds for the sake of their feathers, stopping up all the tracks they made except one, which led to a pit into which they fell, and were suffocated by those that crowded after them. I am thankful to say this custom does not obtain at the present time. On every occasion when I could take the opportunity I used to take up my position upon the rookery to watch for that wonderful silent rush of birds after dusk. It never ceased to charm as well as to astonish.

I will proceed now to give you some statistics regarding the numbers of these birds, based on personal enquiries from those who live by this industry. On Chappell Island on Sunday morning, March 8, after morning service, I collected all the heads of families of the half-castes, and extracted the information I am about to put before you. But first to prepare you for some astonishing figures, I quote what has been said already on this question. Mr. Davies writes that so great were the numbers of the birds returning to Green Island that "night is ushered in a good ten minutes before the usual time." And again—"I have actually sailed through them from Flinders Island to the Heads of the Tamar, a distance of 80 miles." And Flinders calculates that he passed through a flock which must have been 40 miles long. We have fallen upon degenerate days. There are whole islands absolutely deserted now which formerly swarmed with these interesting and lovely creatures. Witness Gun Carriage, or Vansittart Island, as it is called. Not a single petrel ever breeds there now, because of the culpable negligence and cruel thoughtlessness of settlers in old days. They put cattle on the island, and I confess that my indignation rises when I think of the slow agonies endured by thousands of these defenceless young birds crushed to death in their holes under the heavy tread of bullocks. I stand amazed both at the cruelty of such conduct, and at the want of foresight among the farmers. For when once the cattle are regularly placed on one of these islands the birds desert it in a body; and there is no expedient by which they can be attracted back again. So far as it is known they depart never to return. When it is borne in mind that a man and his family can earn about £4 10s. a day for nine weeks in an industry on which he actually spends nothing, which only needs that it should be left severely alone until the moment for catching the birds has come, you will wonder how anyone can be so infatuated as to turn, say, some 20 head of cattle on an island which might otherwise produce 100,000 birds annually, to be sold at an average of 10s. a hundred. This infatuation has not ceased. The islands, some of them, are being ruined at this moment, and I earnestly trust that my voice, together with that of others, will succeed in saving a wonderful industry before it goes the way that the sealing industry has nearly gone, through sheer neglect of the dictates of common sense.

On that Sunday morning I learnt that on Chappell Island (which has an area of 1,200 acres, including rocks and sand patches) there were, in 1890, 21 families at work catching the young birds for some eight weeks, beginning about the 18th day of March. They caught and salted 204,000 birds. I asked—"Did you take them all?" "No, that is impossible. We go over the holes twice, and find just as many the second time as the first. At least 100,000 young birds must escape. When the time comes for their flight the sea is covered with them." Let us consider that 100,000 young petrels then escaped. This brings our total of young birds to 304,000, neglecting those which meet with untimely deaths. Now, each pair of mutton birds lays but one egg. Therefore at the lowest computation (putting aside all barren pairs and those which had lost their eggs or young) there must have been 608,000 old birds. But this is not all. The 21 families of half-castes on Chappell Island live entirely on these birds during their season. I made a calculation with them which showed that from six to ten birds a day is the ordinary allowance for a man; and besides the human beings to be fed there are three or four kangaroo dogs to each hut, who all have to be supported on the same diet. In the two months of their stay these people ate at least 26,000 young birds. And, lastly, the parents of these young ones so eaten amounted to 52,000. So, then, we are well within the mark when we say that on any night in February there were in 1890, on Chappell Island alone, 990,000 petrels. We are accustomed to be told of the wondrous wealth added to our food resources by salmon, which cost nothing but a little watching, and come back from the sea ten times the size they went for the sake, apparently, of feeding mankind. I submit that the mutton birds are a case equally in point. That the young fresh birds are delicious eating I can testify. They taste like a very fresh herring as we know that fish in the Old Country. Broiled or fried it is a dish to set before a king. When salted they are unquestionably nutritious, and probably quite as healthful also, for delicate persons, as cod liver oil. A little barren treeless hummock in our seas produced then last year £1,020 in cash. Destroy that rookery by letting some dozen cattle roam over it and a few sheep and what is the result in the eyes of any reasonable person? What a loss to the wealth of the colony? On this very island there is a farmer who owns 40 acres of land. This he stocks with cattle and sheep. His fence runs right across the track from the only good harbour to the largest cluster of huts. The half-caste lads pull down a panel sometimes. The cattle roam over the rookeries. I have seen them there myself, and I have thought, while viewing the present state of things with regret, how easily the island could be saved from the destruction which is impending.

But to return to numbers. Little Dog Island is in private hands on a Government lease. Three families each take some 27,000 birds off patches of 40 acres in each case. To those interested in the condition of the ground for walking I make the following calculation:—On 40 acres here there must have been 40,000 holes occupied by birds, besides deserted nests. One party on Big Dog Island collected last year 40,000 birds. This island does carry cattle, but it is quite different to the other rookeries. There is a large wood at one end, and the ground is a great deal harder, the birds breeding only at one end of the island. Babel Island probably contains more birds than any of these other places. It is at the S.E. of Flinders. But the snakes are so terrible here that even the half-castes do not like working on it. Two families, however, have attempted it this year. If we take the birds on these four little islands we shall be below the mark when we compute that on any night in February there are two million six hundred thousand sooty petrels asleep upon them.

Perhaps it will interest the members of the Society to hear of another feature of these islands. I remarked that no one on Chappell Island was ever seen without a stick or a gun. It was an ominous sign, betokening the presence of snakes. These creatures do not live apparently on mutton birds, but on mice, which abound there. The young men spend Sunday morning sometimes in killing snakes. Two parties the other day started out to see how many they could get—one party brought in 100, the other 70. Nor did any one in the islands to whom I mentioned this think it an extraordinary case. One man, old Mr. Smith, the half-caste, told me that his party one year threw into a heap the snakes they killed in the two months in the course of their work, and that they numbered 600. When you consider that a man in his prime can take 1,000 birds out of 1,000 holes, in any of which there may be snakes, it indicates the nerve that is needed for this industry. As a matter of fact they rarely touch a snake by accident. There are sure signs of their presence; nor are they often in the holes where the young birds are; and these inhabited holes have also their signs. At the same time a girl of 16 pulled out two snakes in one day a year or two ago; but, then, she was inexperienced. Sleeping on the ground among the bushes on Chappell Island on that Sunday in a hut without a door, and open on every side along the ground, I was not surprised when, after an hour, I heard my companion mutter, "I am too nervous to sleep here: I am off to the boat." For my own part, though I braved it out, I was glad when the first streaks of dawn appeared in the east.

Chappell Island is bad enough, but the half-castes say that Babel is so bad that, though a moderate fortune awaits them there, they do not care to go. I may add that so barren are these hummocks that every family commences operations by stacking wood for two months, loading their boats on Flinders Island, and filling casks of water. There is neither water nor fuel in these places. I have spoken of petrels and of snakes on these islands. I could a tale unfold about another inhabitant, exceeding in numbers even the mutton bird, but I forbear. I merely allude to the dread subject of fleas. I leave to your imagination, which you may actively employ without easily overstepping the bounds of reality, the details of this theme which in that neighbourhood you can by no means put away from your thoughts—and feelings.

What practical proposals, then, can be made for the due protection of mutton birds? I have discussed the question with magistrates in the islands, with all the half-castes, and with white settlers. I think I may say that there is complete unanimity among them all. There is no doubt what should be done. One and all call for restriction. On one occasion, a fine, stalwart fellow, a half-caste, and one of the best boatmen in the world, was urging restriction. What he really said was—"I wish, sir, when you return to Hobart, you would put *strychnine* upon them." What is the sort of "strychnine" we want?

First: The wisest plan would be for the Government to resume possession of the mutton bird islands. Some are now let on lease, such as Little Dog and Big Dog. One is in a sense reserved for the half-castes, namely, Chappell Island. But on this island two white families own small patches of land, which they have bought. In both cases I believe they are honourable men, and have no wish to go beyond their rights. Why should they not be bought out? It would be but a trifling sum, and the islands afterwards would be a regular source of revenue. Some islands, like Babel, are practically untouched by anyone.

Secondly: If resumed by Government, why should not a license be taken out by every person who wishes to "bird"—so much for an adult, say, 10s., and a lesser sum for the younger members of the family? At least £100 a year could be received at once from this source of revenue. But even if the Government does not resume possession (and I, for one, earnestly trust they will take them entirely into their own hands), still the points, which are simply dictates of common sense, are these—

(*a.*) That there be a close time for these birds, just as there is a close season for quail: At present there is none.

(*b.*) That no egging be permitted. The waste in past years, and even at this time, is terrible, and there is practically nothing gained by the taking of the eggs, whereas the number of birds is materially lessened. Formerly they were eaten in large numbers. Mr. Davies states that an officer of the old settlement on Flinders saw a woman eat 52 eggs in one day. As they are as large as ducks' eggs that was a first-rate performance.

(*c.*) That a fixed day should be gazetted before which no one should be allowed to touch a bird. Persons differ whether it should be March 15 or any date up to March 20. There is great need for such an order. Families on Chappell Island have no recognised spaces on the rookery. They begin upon the same day, each near his own hut, and work outwards and upwards as they please. If one man begins too early the others are compelled to work in self-defence. There is no need to fix the close of the season. The birds arrange that by flying clean away about the middle of May, and not returning till September.

(*d.*) No fatting should be permitted. Fatting means taking the immature bird before it is ready for salting, and boiling it down for the oil. As 200 birds make about a gallon or so of oil, which brings 3s., and as those 200 birds a week or two later would sell when salted for £1, the unwisdom of the former operation is obvious.

(*e.*) No cattle or sheep should be permitted on these islands. Big Dog Island may be an exception, but there is no question the other little islands should be protected. I know how many in this colony are enrolling children in Bands of Mercy in order to teach them to be gentle and kind to all animals. I have heard of a "Dicky Bird Society," which exists for similar purposes. I invoke the aid of all such in the cause of the gentle and inoffensive "yolla"—that is the native name for the sooty petrel or mutton bird. I invoke the aid also of all who have been fascinated by the exquisitely graceful flight of these birds as they soar and wheel round the passenger steamers that ply round our coasts. The mode of killing them for the industry is painless; one twist of the neck and they are dead. A single pressure on a gland in the stomach after death and the oil escapes at the mouth and is caught in a vessel. This is legitimate, and not barbarous, for we must kill in order to eat. But it shocks all our finer feelings to think of cattle trampling upon hundreds of young birds, which die and rot uselessly in their holes.

(*f.*) An order should be issued against fires, except between May and September. It is, of course, difficult, but not impossible, to get the grass to burn in winter. At any other season it means ruin to the industry and death to the petrels.

(*g.*) Chappell Island, in consequence of the cattle that have been on it, would be the better for a year of rest. All agree as to this. But the question arises, "Where shall the half-castes go for this one year?" Could they attempt Babel? Could they go to some of those islands off Wilson's Promontory, which are said to swarm with birds, and no one touches them? I am told there are not even boat harbours in them. At any rate, Chappell Island, as being in some sense reserved for the half-castes, merits our chief attention. There need be no difficulty in carrying out these arrangements. Under the direction of the worthy magistrates in the islands and the well-known police constable everything could be at once arranged. If it is asked why such obvious regulations have not been made long ago, I answer that perhaps the chief reason is that never have these islanders recorded a vote for a member of Parliament. There has been no polling station, no registration office for them. I have wondered why a would-be member for Ringarooma (in which district the islands are included) has never bestirred himself to get fifty solid votes from the Furneaux Islands, votes which would be gladly given to anyone who would advocate their just claims and protect their industries.

If anything I have said leads to a greater interest in one of the most beautiful of our sea birds, and indirectly helps to foster a useful industry, and further the cause of humanity in the protection of the "yolla" or sooty petrel, through the instrumentality of the Royal Society, I shall have deemed it an honour as well as a privilege to have put this paper before you this evening.

DISCUSSION.

Mr. MORTON said that the value of the mutton bird industry had never been so prominently brought before the notice of the Society since Mr. Davis read a paper on the subject, which was printed in the Tasmanian journal. He looked upon it as a matter which might very well engage the attention of the Fisheries Board. He would very much like to find out from statistics whether this wholesale destruction of the petrel had decreased the numbers. As far as he was able to gather they had not decreased. It was plain, however, that were cattle were introduced the birds had disappeared and gone to some other place. Viewed as an important industry, he thought these birds should be protected, especially as it was thought necessary to protect quails, which brought no revenue into the country, but merely provided sport.

Mr. Bernard Shaw said that there was already an Act of Parliament enabling the Executive to deal fully with the matter. It was passed last session, and there did not appear to have been sufficient time to prepare the necessary regulations. Under that Act the Governor-in-Council was empowered to make regulations fixing a close season for the birds, preventing the introduction of cattle and sheep to certain islands, the lighting of fires, and various other matters to which His Lordshp had referred in his paper. He considered that interference in these matters was beyond the functions of the Fisheries Board. The Act to which he had referred gave ample powers to make all the regulations necessary, and he hoped that before next season there would be a complete set of regulations in operation.

Mr. Alfred J. Taylor said that before Bishop Sandford left the colony he brought this matter under the notice of the Society, and pointed out that a valuable industry was being destroyed. He had also referred to the fact that the oil of the bird had been found valuable as a medicine in cases of consumption.

Mr. R. M. Johnston said that having visited these islands on several occasions he could corroborate His Lordship's description of the return of the birds. It was a glorious sight to see the multitude of birds returning, and he was glad to see that steps were being taken to see what could be done to protect these birds. In regard to the destruction of the birds, he thought this arose not only from the destruction of the birds, but also of the mounds in which they burrowed.

His Lordship briefly replied, expressing pleasure at the fact that legislation had been initiated in the direction indicated, and hoping that the necessary regulations would be framed to protect what he regarded as an important industry.

NOTES ON A COLLECTION OF PLANT IMPRESSIONS FROM THE HENTY RIVER.

By R. M. Johnston, F.L.S.

Lower Coal Measures.

Until very recently we were ignorant of the existence of the extension of members of the Carboniferous Rocks to the west of the Eldon Bluff. About six months ago I received a collection of fossils from Mr. T. P. H. Jones, taken from a deposit extending eastward along the northern side of the Henty River, commencing at its junction with its tributary, The Badger, and apparently occupying a patch of about 4 miles square, lying between these rivers eastward of the fork. Mr. Jones states that he has traced some of its members forming a narrow curved strip bending northwards towards, and nearly approaching the vicinity of Mount Dundas.

Mr. Montgomery also visited the locality at this time, and we may hope to learn from him further particulars regarding the stratigraphic relations of the various members of this deposit at an early date. Having expressed to Mr. Jones my desire to obtain a more complete collection of the plant impressions, he very kindly undertook to do so, and recently, through the kindness of Mr. Meiring, I received a collection of Lower Coal Measure plants, containing some of the finest examples of *Glossopteris Browniana* that I have yet seen in Tasmania. One of these is nearly perfect, and must have been originally 14 inches in length, having a breadth of $2\frac{7}{8}$ inches at its greatest diameter.

Another fine example of *Glossopteris,* which I regard as a new species, is 9 inches in length, and 4 inches broad at its greatest diameter. These beautiful ferns are very remarkable, and are associated with abundant remains of the following species—all of which also occur in the shales of The Lower Coal Measures at the Mersey River: viz.:—

Gangamopteris spatulata. M'Coy.
——————— obliqua. „
Noeggerathiopsis media. Ettings.

The shales in which these plants are imbedded are hard and fissile, and of a very dark grey colour. They contain many curious botryoidal concretions; some rounded or nut-shape, and all of them having a bright glazed surface. I am yet unable to account for them.

Intimately associated with these shales occur fossiliferous mudstones and impure limestones identical with similar deposits in the Mersey, and throughout the eastern part of Tasmania.

The more numerous fossils in these beds are the following:—

Spirifera Tasmaniensis.
——— avicula.
——— convoluta.
Sanguinolites Etheridgei.
Fenestella internata.
——— plebeia.
Protoretepora ampla.
Stenopora Tasmaniensis.

There can be no doubt, therefore, that these beds are the equivalents of the Coal Measures of the Mersey, and there is some hope that Mr. Jones may be successful in his search for coal in this neighbourhood.

LIGNITE BEDS.

Mr Jones informed me that the Coal Measures at the Henty are flanked by beds of lignite, and among the specimens sent to me were several small fragments of lignite, no doubt obtained from the same place. On examination I was successful in obtaining casts of two or three new leaf forms. One of the forms is undoubtedly a species of *Fagus* with very minute leaves, closely allied to the existing *Fagus Cunninghami* of Tasmania. Another is a well-preserved impression of the phyllodium of a species of Acacia, closely resembling the existing *Acacia melanoxlyon.*

Descriptions of the principal forms of the Lower Coal Measures, together with these of the Tertiary lignites, are given hereafter.

One of the forms I have named *Fagus Jonesii*, in honour of Mr. Jones, to whom we are indebted for their discovery. Another has been named *Acacia Meiringii*, in honour of Mr. Meiring, to whom also I am indebted for placing such a fine collection at my disposal. The close resemblance which these two forms bear to existing species, incline me to the opinion that the lignites at this place are of a more recent date than any other lignite formation hitherto described.

GLOSSOPTERIS BROWNIANA.

Frond lanceolate-elongate, obtusely acuminate; costa thick, gradually diminishing, but extending to the apex; veins, close parallel, ascending from costa acutely, at an angle of about 50 deg., and gradually curving outwards, where they emerge at an angle of 30 deg. Anastomoses, long and narrow, only slightly broader close to the mid-rib. Large specimen, 14 inches long; breadth at greatest diameter, $2\frac{7}{8}$ inches. Locality, dark shales, associated with Carboniferous Marine Beds, near junction of Henty and Badger River.

Glossopteris ovata. *Nov. sp.*

Frond, broadly-ovate; margin, slightly undulating; apex rounded, not produced; costa, moderately thick towards base, diminishing, but extending to apex; veins, closely parallel, diverging from costa at an angle from 11 deg. to 22 deg.; anastomoses, obscure, but do not appear to be frequent. Specimen, 9 inches long, greatest breadth at middle of frond, 4 inches. The broad, rounded form, obscure anastomoses, and the low angle at which the sharp veins diverge from mid-rib, seems to justify the separation from G. ampla to which it is most closely related.

Locality.—Henty River Lower Coal measures.

Fagus Jonesii. *Nov. sp.*

Leaves, very minute, shortly petiolate, broadly ovate or deltoid, flat, or slightly convex, coriaceous: margin, strongly dentate: mid-rib, somewhat prominent: primary veins, scarcely visible. Lignite Beds, Henty River.

Acacia Meiringii. *Nov. sp.*

Phyllodium, lanceolate: apex obtuse, narrowed towards the base; slightly coriaceous, with five fine longitudinal nerves.

Anastomosing veins appear to be much closer and finer than in the existing *A. melanoxlyon* with which this form seems to be closely allied.

Length, $3\frac{7}{8}$ inches; breadth at middle, $\frac{7}{8}$ inch.

Locality.—Lignite Beds, Henty River; associated with leaves of extinct species.

BRIEF REMARKS ON SOME RARE TASMANIAN PLANTS.

By Baron Von Mueller, K.C.M.G., M.D., Ph. D., F.R.S.

Coprosma Petriei, Cheeseman in the transact. of the N.Z. Institute, XVIII., 316 (1886).

Under this name I wish to bring under notice what appears to be a new Tasmanian Coprosma, lately found as of rare occurrence by Mr. T. B. Moore on the highlands east of Mount Tyndall. It has the same very depressed matted growth as C. repens (C. pumila), also very small leaves and terminal small-sized fruits. But the leaves in all the specimens received are decidedly pointed, indeed ovate-lanceolar, and the fruit is beautifully blue outside, a characteristic which separates this species from all other Australian kinds, and which is not likely subject to variation. Mr. Thomas Cheeseman in his excellent review of the 31 New Zealandian species of this genus distinguished by him, mentions two as having fruits blueish outside, namely C. parviflora and C. acerosa, the former otherwise very different from our plant, the latter of much larger size, with puberulous branchlets and longer but narrower leaves. Nevertheless C. Petriei is described as varying in the outside colour of the fruit, red in the Nelson, blue in the Otago province, but possibly two species became thus confused, in which regard already some indications are given in the transact. of the N.Z. Inst. XIX., 251 and 252. As the flowers of this plant are not yet known, it remains for some future opportunity to confirm the differences existing in this respect between C. repens and C. Petriei. The fruits are globular or verging into an oval form; so far as seen on this occasion they ripen only one, rarely two seeds. The embryo is only half as long as the albument. Should the Tasmanian plant, after the flowers have become known, prove a peculiar species, then such ought to be distinguished under the finder's name.

Panax Gunnii—

The fruit of this rare shrub was also for the first time obtained for me by Mr. T. B. Moore, who gathered it in deep shady gorges at Mount Lyell, on the Canyon River, the Franklin River and on a tributary of the Pieman's River. It is succulent, about $\frac{1}{3}$-inch broad, renate-roundish, turgid, black outside, at the summit five-denticulated and impressed, so that the styles are hardly visible; the two nutlets inside are oblique-ovate or demidiate-roundish, about $\frac{1}{6}$-inch long, rather turgid, exteriorly grey-brown and nearly smooth. This plant seems to bear flowers already, when only 6in. high, and never to exceed 4ft. in height, unless perhaps in cultivation.

Styphelia Milligani—

Under this appellation occurs the Pentachondra verticillata in the second systematic Census of Australian Plants p. 178, in anticipation of the fruit proving that of a Styphelia (or Leucopogon), a surmise fully borne out by specimens sent by Mr. Moore from the highlands of Mount Read and Mount Tyndall, where also a small form of Acacia mucronata is growing at elevations between 3,600ft. and 3,900ft. The fruit, as now seen, is only of about $\frac{1}{8}$-inch measurement, nearly globular; its pericarp is very thin and outside white; the putamen is five-celled. Possibly the fruit obtained may be over-aged. Until now the plant was only known from Dr. Milligan's collection. It is from 6in. to 18in. high, but as it is many-branched from the root, Mr. Moore saw individual plants covering a breadth of 2ft. When out of flower this plant calls to mind, as regards its aspect, some Pultenæas.

It may here not be inappropriate to remark that since Sir Joseph Hooker finished, in 1860, his superb work on Tasmanian plants, the following were by me brought under notice as additional among vasculares (they coming within the scope of my own researches) as the Tasmanian flora could not be kept apart in treating that of Continental Australia, some few only emanating from other collections:—

Papaver aculeatum, Thunberg.
Cakile maritima, Scopoli.
Pittosporum undulatum, Andrews.
Comesperma defoliatum, F. v. M.
Elæocarpus reticulatus, Smith.
Pseudanthus ovalifolius, F. v. M.
Euphorbia Drummondi, Boissier.
Casuarina bicuspidata, Bentham.
Zieria cytisoides, Smith.
Zieria veronicea, F. v. M.
Eriostemon Oldfieldi, F. v. M.
Atriplex paludosum, R. Brown.
Polygonum lapathifolium, Linné.
Acacia penninervis, Sieber.
Acæna montana, J. Hooker. (Recorded as a variety in the Fl. Tasm.)
Pimelea Milligani, Meissner.
Pimelea stricta, Meissner.
Pimelea axiflora, F. v. M.

Pimelea serpillifolia, R. Brown.
Eucalyptus Sieberiana, F. v. M.
Eucalyptus Stuartiana, F. v. M.
Panax sambucifolius, Sieber.
Hakea ulicina, R. Brown.
Hakea nodosa, R. Brown.
Coprosma Petriei, Cheeseman.
Cotula filifolia, Thunberg.
Calocephalus citreus, Lessing.
Cassinia longifolia, R. Brown.
Podosperma angustifolium, Labillardiere.
Ixiolæna supina, F. v. M.
Leptorrhynchus nitidulus, De Candolle.
Helichrysum Spiceri, F. v. M.
Helichrysum Gravesii, F. v. M.
Anaphalis Meredithæ, F. v. M.
Lobelia platycalyx, F. v. M.
Lobelia rhombifolia, De Vriese.
Lobelia Browniana, Roemer and Schultes.
Lobelia microsperma, F. v. M.
Lobelia pratioides, Bentham.
Leeuwenhoekia dubia, Sonder.
Donatia Novæ Zelandiæ, J. Hooker.
Scævola æmula, R. Brown.
Scævola microcarpa, Cavanilles.
Goodenia barbata, R. Brown.
Styphelia elliptica, Smith.
Styphelia scoparia, Smith.
Solanum vescum, F. v. M.
Veronica plebeja, R. Brown.
Veronica notabilis, F. v. M.
Westringia rosmariniformis, Smith.
Verbena officinalis, Linné.
Myoporum parvifolium, R. Brown.
Prasophyllum nigricans, R. Brown.
Pterostylis vittata, Lindley.
Orthoceras strictum, R. Brown.
Caladenia suaveolens, G. Reichenbach.
Thismia Rodwayi, F. v. M.
Milligania Johnstoni, F. v. M.
Potamogeton perfoliatus, Linné.
Potamogeton Cheesemani, A. Bennett.
Potamogeton pectinatus, Linné.
Zostera nana, Mertens and Roth.
Lepyrodia Muelleri, Bentham.
Calostrophus elongatus, F. v. M.
Schœnus Tepperi, F. v. M. (or a closely allied species.)
Heleocharis acicularis, R. Brown.

Gahnia Radula, F. v. M.
Carex tereticaulis, F. v. M.
Carex Bichenoviana, Boott.
Sporobolus Virginicus, Kunth.
Agrostis frigida, F. v. M.
Agrostis Gunniana, F. v. M.
Zoysia pungens, Willdenow.
Imperata arundinacea, Cyrillo.
Cyathea Cunninghami, J. Hooker.
Blechnum cartilagineum, Swartz.
Asplenium Hookerianum, Colenso.
Aspidum hispidum, Swartz.
Hymenophyllum marginatum, Hooker and Greville.
Hymenophyllum Malingi, J. Hooker.

In the concluding pages of the "Flora Tasmaniæ" were already inserted solely from Melbourne communications as additional.

Kennedya monophylla, Ventenat.
Geum renifolium, F. v. M.
Aciphylla procumbens, F. v. M.
Leptomeria glomerata, F. v. M.
Abrotanella scapigera, F. v. M.
Senecio primulifolius, F. v. M.
Senecio papillosus, F. v. M.
Dracophyllum minimum, F. v. M.
Sebæa albidiflora, F. v. M.
Limnanthemum exigeum, F. v. M.
Dendrobium striolatum, G. Reichenbach.
Selaginella Preissianum, Spring.

THE POSSIBILITIES OF THE TELESCOPE.

By A. B. Biggs.

Up to about the end of the 15th century mankind was in a condition of helpless ignorance with regard to the nature, the distances, and the dimensions or (except two) the form of the various bodies which constitute the visible universe. No human eye had ever beheld either stars or planets as other than dimensionless points. Their motions and positions had been carefully observed, systematised and theorised upon; but they were unapproachable. What a wonderful revelation then must that have been which *Galileo's telescope* opened up! practically diminishing the distance some 20 or 30 times. True, his was a very simple affair, and feeble in its infancy; but it revealed some most important facts. It served to show that the planets at least were globes, some of them of vast bulk: it opened up to human vision for the first time the wonderful mountainous scenery of the Moon. But perhaps its most important service was to establish beyond question the Copernican theory of astronomy by revealing the phases of the inner planets—also the moons of Jupiter—a Copernican system in miniature.

The Rings of Saturn constituted an inscrutable riddle, reserved for further development of this new power of vision.

Since Galileo's time, the effort has been constant and unwearying to develop to the utmost the power of the instrument. There were difficulties inseparable from the principle on which the instrument is constructed, which it was long thought never could be overcome, the chief of which was (briefly stated) that a ray of light, when bent out of its course, as by a prism, or a lens, is separated into its different component colours, each having a different focus. The discovery and utilisation of the fact that different kinds of glass have different separating or prismatic effect, led to the construction of the *achromatic* object glass. This gave a new start to the powers of the instrument, so that we have reached from Galileo's power of 30, and imperfect at that, to a power ot about 3,000 in the Lick Telescope.

Well, now the question arises:—If we have from the time of Galileo been enabled by the gradual improvement of the telescope to stretch our gaze farther and farther into space, why should we not still go on enlarging its scope. As we now see clearly the configuration of the lunar mountains, why should we not, in time, come to see the trees growing upon there sides, if there be any? Why not discover signs of organised existence, if such exist? Is there any limit to which the instrument is capable of being developed. This is the question which it is natural to ask, and which I have been asked repeatedly. On this account I have thought it might be deemed an interesting question to discuss.

I may state at starting that doubtless there *is* a limit, and moreover, I believe that limit is already nearly, if not quite, attained. To prepare the ground for a clear understanding of my reasons, I must very briefly refer to the fundamental principles of the telescope. A luminous body, such as a star, emits rays diverging in all directions in straight lines. Of course the greater the distance the more these rays are spread ; that is, the light is diminished in intensity, and that in proportion to the *square of the distance.* Of the total light emitted that received by the eye is that fraction which the pupil bears to the whole sphere of which the radius equals the distance. These rays may be so enfeebled by distance that the object ceases to be visible. Now, if we can gather a sheaf of these rays that would otherwise pass by us unperceived, and bend them inwards until they enter the pupil of the eye, we evidently increase the visible luminosity in that proportion. This is the function of the object glass—*i.e.*, the large glass at the outer end of the telescope—or the speculum of the reflecting telescope (See Fig 1).

Of course, the larger the glass the more it can gather. As the rays from every point of the object are brought to a corresponding point at the focus of the lens, a small image of the object is formed. This image is magnified by the eyepiece, which is really a *microscope.* But whether we magnify this image less or more, we have only the same amount of light to deal with, viz., that which is grasped by the object glass ; and the more this light is spread out by magnification the more it is enfeebled. This magnification is therefore limited by the capacity or size of the object class—(other things being equal). In this respect the Telescope and the Microscope stand on a very different footing. We cannot, of course, increase the actual luminosity of a celestial object in the smallest degree. All we can do is to grasp as much as possible of the light actually emitted. With the microscope, on the other hand, we can, by means of condensers, and the employment of suitable sources of light, increase the illumination of the object indefinitely up to the required amount of amplification. Hence it is evident that increase of *telescopic power* is to be sought in the *enlargement of the light gatherer*—the object glass. But this is where the trouble comes in. The difficulty of accurately figuring a large object glass, or speculum, increases enormously out of proportion to its dimensions. If every point of its active surface does not refract or reflect its ray to its exact point in the focus, such defective part is worse than useless. To give some idea of the nicety of manipulation required, I may state that a speculum is ground and finished up to the finishing touch of fine grinding, as a portion of the surface of a *perfect sphere.* But this figure would not do at all for the telescope. The marginal rays would be reflected to a different, that is, a *shorter* focus than the central ones. The radius of curvature needs to increase as it approaches the margin. The figure needs to be *parabolic*, answering nearly to the end of a long ellipse. But so little does this parabolic figure differ from the spherical, that it is all *done in the polishing.* And this is especially where the highest skill of the manipulator is called for. An extra rub with the palm of the hand may be sufficient to distort its figure

Then another trouble comes in, increasing enormously with the size. We cannot get size without an immense increase of *weight*, and then comes the difficulty of supporting in their tubes these heavy masses without flexure or straining, the least degree of which is fatal to definition.

But now, supposing all these difficulties successfully overcome, there remains yet another obstacle, which is utterly beyond man's power or skill to contend with. We live at the bottom of an ocean of air; and we could not exist even if we could surmount it, any more than a fish could live out of water. And this ocean is in a state of ceaseless agitation, more or less. Through this medium must pass the rays which go to form the telescopic image. Some idea of the effect of this may be formed by trying to read the inscription on a coin lying at the bottom of a clear pool that is agitated by the breeze. I have viewed the planet *Saturn* in the telescope, when, from this cause, it presented the appearance of a fire-ball; huge tongues of flame apparently blazing all round it, flickering and dancing; precisely like those *kerosene fire-balls* which the boys throw up into the air with such magnificent effect on Queen's birth-nights. It was, to all appearance, a *world in a blaze.* It is not then to be supposed that we can, whenever we please, obtain a clear and distinct view of any celestial object we may wish to inspect. Favourable occasions for this are few and far between. Sometimes a lull in the atmospheric waves permits of a momentary glimpse of the object clearly and sharply defined; just as, in the case of the coin in the pool, a brief lull in the breeze allows you to begin to read the inscription; but before you get half-way through another puff comes up and blurrs it all over.

Now this difficulty increases with the size of the telescope, inasmuch as that the larger diameter takes in a greater number of these disturbing atmospheric waves; for which reason the observer has often temporarily to reduce the effective aperture, that is, the *power* of his telescope.

The only possible amelioration of the obstacle referred to is to leave as much as possible of this disturbing medium below the observer. This is just what is done in the case of the great *Lick Telescope*, which is erected on a mountain in San Francisco at an elevation of 4,300 feet, or about the height of the top of Mount Wellington. This, coupled with its vast size and perfect finish, places this instrument in the advance post of telescopic power. It has lately been proposed in France to erect an Observatory on one of the Alps, at an elevation of 15,000 feet. Owing to the compressibility of the atmosphere by the superincumbent weight of the upper strata the half of the entire mass lies somewhat below an elevation of three miles, although the rarer air probably extends considerably over 100 miles. But the necessity of air and warmth places a limit not far to reach in this direction. The most daring of balloonists ventured up to an elevation of about seven miles (I speak from memory), when one of the occupants became insensible, and his companion had only strength left to release the gas with his teeth, having lost the use of hands, and descend into a denser atmosphere.

Assuming that I have fairly stated the case, it would appear then that the smallest advance in telescopic power is necessarily to be obtained only at a cost enormously out of proportion to the gain, or, as it is commonly expressed, " the game is not worth the candle."

One word with reference to the announcement of a new optical glass for the manufacture of telescopic lenses, about which a deal of tall talk has been indulged, and probably not a few hoaxes, with more yet to come. This sort of talk springs from an entire ignorance of the function of any such glass and a supposition that some magical magnifying power is inherent in the glass itself instead of in the skill required to shape it. The function of such glass is simply to combine more accurately the different coloured rays, which existing combinations fail, to some extent, to bring to a common focus, the result being more perfect definition.

A brief explanation will, I think, serve to make this clear. *Light* passes in *straight* lines through a uniform medium. On entering a denser transparent body the ray is bent towards the substance of that body (See Fig. 2).

But what we see as *white* light is really composed of the blending of all the colours of the rainbow, together with some other rays, viz., the actnic or chemical rays and the heat rays. Now, these latter, together with all the different colours, are unequally " refracted " or bent. The light is, in fact, decomposed. Of the *light* rays the *violet* is the most bent, the *red* least. They are thus spread out into a band, along which the different colours are distributed. A simple lens, such as that of the primitive telescope, is *prismatic*. The effect is that the red rays are brought to focus at R, and the voilet to a shorter focus at V, the other tints lying between. Thus there is no definite focus (See Fig. 3).

We have, then, two effects from the passage of a ray through a *prism*—the *bending* or *refraction*, and the *decomposing* or *dispersion*. But the grand discovery was made that different kinds of glass *disperse* or spread the colours in different degrees with a given amount of refraction, thus:—If the former prism had been of *flint* glass instead of *plate*, and made to give the same amount of bending, we should have had the different colours spread over, perhaps, double the space. To produce an equal spreading of the colours we shall require a *flint* glass prism that will bend or refract *less*. We shall require to bend the ray only to B instead of to A (Fig. 2); that is, we shall require a thinner or less angular prism. Now, place these two prisms together in reverse positions, and what will be the effect? The ray will be partially brought back towards the straight, viz., to C instead of to A (See Fig. 2 b), leaving still an amount of deviation, from the greater thickness of the *plate* glass. But the dispersion of the colours is in *reverse* order in consequence of the inversion of the prisms, but in equal degree. The complimentary colours are therefore superimposed, and are re-composed into *white light*. This is the secret of the vast advance in telescopic power since the days of Galileo.

Now to apply this to the telescope. The lens (*P*) of *plate* glass (see Figure 4) is backed by a concave of *flint* glass (*C*) of less refraction power, thus leaving a surplus of refraction to the convex, the effect being that we get a focus at *F* instead of at *f*, with all the different coloured rays brought to a common focus.

Now, there is one trouble that has not yet been surmounted. Although the *general* prismatic dispersion may be equalised and neutralised, the colours are *unequally* distributed by the two kinds of glass. Consequently one particular coloured ray does not meet its exact complimentary in the reversed spectrum, and as a result we have a fringe of outstanding colour. This is called the "irrationality" of the spectrum. This is what opticians are endeavouring to get rid of by the invention of a glass that will locate the different colours in the same relative position as does the plate glass of the convex, and this is all the *magic* there is about it.

POSSIBLE POWER OF THE TELESCOPE.

I think many persons entertain very exaggerated ideas of what the telescope in its present stage can really accomplish. Theoretically it is assumed that the utmost available power of the most perfect instrument, and under the most favourable circumstances as to the state of the air, etc., is 100 to the inch of aperture. This is the *extreme*, rarely reached. The Lick Telescope has an object glass 36 inches in diameter. Considering the necessarily immense thickness of glass through which the light has to pass, we may assume 3,000 at the utmost of its power. This should, if definition were perfect, give us a view of the moon as at a distance of 80 miles. Of course this is a vast approach. But what could we see of the details of a terrestrial mountain, supposing we could discern it at 80 miles distance? This estimate is, of course, on the supposition that we could eliminate all optical imperfection and atmospheric disturbance, both of which are impracticable. In connection with this question I made the following experiment the other day:—I approached a handbill, of the subject of which I was ignorant, until I could just make out the boldest type. I found the distance 16 yards. The letters were bold block letters, 1 inch in height. Reckoning from this, similar letters on the moon's face, to be seen by natural unassisted vision, should measure 417 miles. There would be room across her face for a word of five letters. With a telescopic power of 3,000, and no imperfection introduced, such letters, to be readable, would have to be 244 yards. Each letter would thus cover a space of about 12 acres. Allowing for imperfection, I think this would be the smallest patch that would be discernible at all as to shape. At the distance of Jupiter this object would need to be about four million times larger in surface, or, say, three times the size of Tasmania.

PHOTOGRAPHY.

There is one direction, however, in which the powers of the telescope are in process of development to a considerable extent, that is, by its association with photography. I have so far dealt with the telescope as a *seeing* instrument. Whatever is to be seen is seen at once. We cannot increase its vividness or distinctness by prolonged gaze. If a faint star, *e.g.*, is not at once visible when the eye is directed to its position, any amount of staring at it will not bring it into view. Not so with *photography*. The more it looks the more it sees. In this way it has been arranged to explore the entire heavens to depths far beyond the limits of telescopic vision by combined effort of all the principal Observatories in the world. Already vast numbers of minute stars have impressed themselves upon the photographic plate that would probably for ever have remained beyond the power of actual vision. To accomplish this something more is necessary than merely focussing the object upon the photo plate. It has to be kept there accurately during the whole time of the exposure—a needle's point to a spider line—and that for hours, against the diurnal motion of the earth. To give the proper motion to the telescope the most accurately constructed driving clock-work is necessary. For planetary detail, however, I much question if photography will ever render much service. There is no getting over the blurring caused by atmospheric disturbances, superadded to instrumental imperfection. The photo of moon from the great Melbourne telescope is generally accounted one of the most successful ever produced. But in this, minute details that are distinctly visible in either of my comparatively small telescopes are totally absent.

A few remarks on the necessity of *caution* in rightly interpreting telescopic appearances may fittingly conclude this paper. A novice on taking his first peep at the planets, we will suppose, will probably feel quite disappointed that the object he sees exhibits very little of the detail or appearance of that same object as depicted and described by experienced observers, and after long and diligent observation. The eye requires considerable training for this kind of seeing. Moreover, considerable judgment is necessary in rightly interpreting what we see.

ERRATA (IN PLATE).

Omit the words "Page A," etc.

"In FIG. 2 *b* add a dotted line (A) from prism, above C."

Figures for

Page 3 "Possibilities of the Telescope.

FIG. 1

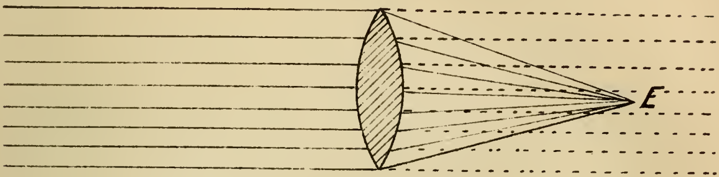


FIG 2,
Page A.

FIG 2^{b}
Page A.

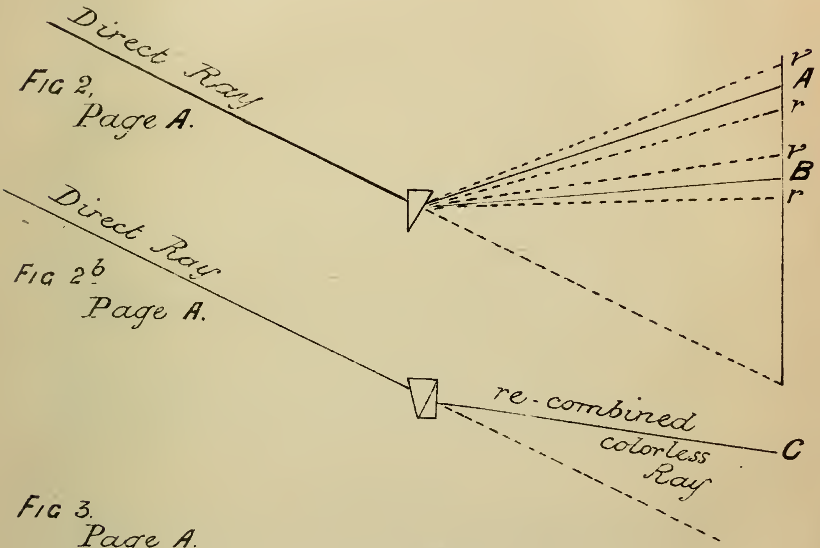


FIG 3.
Page A.

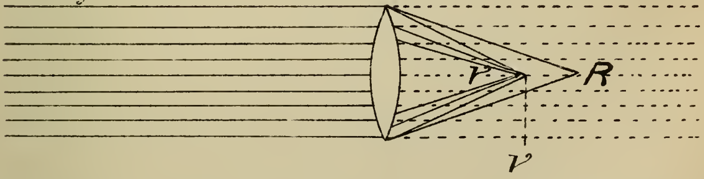
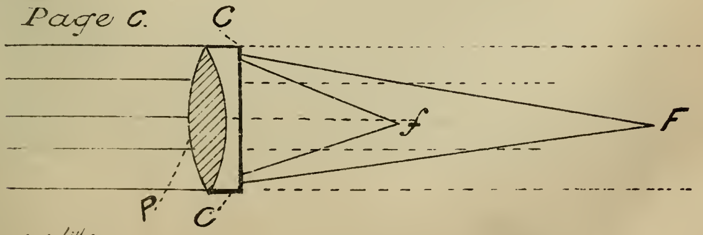


FIG 4.
Page C.



Mercury. litho.

CENTRAL PARK
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NOTES ON THE MOUNT LYELL DISTRICT, TASMANIA.

By Frederick Danvers Power, F.G.S.

(Corresponding Member Royal Society of Tasmania.)

On taking a casual glance at the geologically coloured map of Tasmania we are struck with its similarity in appearance to some huge concretion, for we see a nucleus of greenstone, which is surrounded more or less by a ring of the upper coal measures, and these again by rocks from other epochs.

If instead of the geological map we take a topographical one we will observe the same onion-like structure. In the centre we find the Lake Plateau country, from which the river systems of Tasmania radiate. Encircling this are various ranges of mountains, and finally we have the sea coast, which has practically the same contour as the kernel, making due allowance for irregularities, which would be exaggerated by enlargements. Looking at the island as a whole, it is heart-shaped, and somewhat similar in outline to the continents, inasmuch as it is widest at the northern and narrowest at its southern end, its length being north and south. The geological and topographical features coinciding so closely, we are naturally led to the conclusion that they must in some way or other be intimately related. If we can further connect our ore deposits with the physical developments of the country we may gain something by our studies with which the strictest utilitarian cannot find fault.

Orography.

Mr. R. M. Johnston supposes that many of the Tasmanian mountains are due to the shrinkage of the earth's crust; but it has long since been proved that the elevations of our mountains are far in excess of that required by the cooling of the world, and although some slight puckering of the surface may be due to this cause, still I believe it will be generally conceded that other agencies are also at work, and that the presence of the ocean is the greatest of these.

We almost invariably find that the highest mountain ranges have been developed more or less parallel to the past or present sea coasts that are washed by the largest bodies of water. Thus in South America we find the Andes on the west higher than the mountains of the east coast; also that the western shores of America are washed by the Pacific Ocean, which is greater in area and depth than the Atlantic, which is on the eastern coast. Coming nearer home for an example, we find the highest Australian mountains on the east coast, the Pacific again being larger than the Indian Ocean. In Tasmania, however, matters are somewhat different, for there is an unbroken stretch of water from its west coast to South America; while on its east coast, some twenty degrees distant, New Zealand acts as a breakwater, and braces up the ocean, so to speak, relieving the east coast of Tasmania of much pressure from the main body of water. Therefore we are not surprised to find that the backbone of Tasmania is on the western side, especially when we find a map in Wallace's "Island Life" showing the deep water on the west coast at 2,600 fathoms, against 2,000 fathoms on the east coast. We not only have the horizontal pressure of the ocean, but also the vertical pressure, and the force is not a steady one, but varies with the tides, this movement being more effective in shifting the earth's crust than a dead pressure. The pressure of the immense bodies of water in the oceans are working as hard to-day in the building up, and indirectly in the pulling down, of our mountain systems as they have in the past, and it makes its pressure felt second-hand by the enormous weight of the rocks that they pile up, which in their turn exert a vertical pressure. We generally find that the actual coastal range is not so high as the next inland one, as if the force causing the mountain system had limited the area on which the range nearest to it could draw; also that the ranges following the main one are lower and further away from each other as they go inland. So in the case of Tasmania we find the West Coast Range comparatively low. Then comes the Main Range, including the Frenchman's Cap, Eldon Range, and the Cradles, which are among the highest mountains of Tasmania. These are followed up further inland by the King William Range. These ranges are like immense fossil waves rolling inland. They have a long slope on their western side and a steep one on their eastern, just as if they had been pushed up by the sea. Having taken a flying survey of the physical features of Tasmania we will now make a few remarks relative to the mineral wealth of the West Coast, and more particularly to the Mount Lyell district.

GEOLOGY.

The West Coast of Tasmania is rich in the variety of its minerals, among which ores of the following metals are found:—Tin, gold, silver, lead, zinc, copper, iron, chromium, nickel, bismuth, wolfram, iridium, antimony, manganese, and mercury; also other minerals of commercial value—*e.g.*, marble, serpentine, asbestos, lignite, etc.

Mount Lyell (named after the celebrated geologist, Sir Charles Lyell)—and its district is a most interesting one from a geologist's point of view, for here we have two or three different types of ore deposits. The most widely known, perhaps, is that called the "Iron Blow," on account of the hugh boulders of hematite (specimen 556) outcropping on the surface. Mounts Lyell and Owen (named after Professor Owen) are two peaks of the West Coast Range, divided by the Linda Valley, but connected on their western slopes by a saddle, which forms a watershed, the drainage on the east flowing down the Linda into the King River, and that on the west finding its way into the Queen River. That portion of the Iron Blow worked by the Mount Lyell Gold Mining Company is situated on the eastern side of the Mount Owen end of the saddle. The country rock about here consists of sandstones, green—(specimen 546) and red (specimen 551), bleached at the surface (specimen 553); various schists—ferruginous (specimen 550), talcose (specimen 548), hydro-mica (specimen 547), etc.; and limestone (specimen 552), which has a schistose structure. The general strike of these rocks is approximately parallel with the West Coast Range, and also the sea coast in these parts. The older rocks are overlaid with a quartz conglomerate (specimen 549), which caps the highest points of Mounts Lyell and Owen.

The Iron Blow Deposit.—Mr. G. Thureau believes this lode to be due to thermal action, but after carefully going over the ground three or four times I failed to find any facts which would confirm this theory.

Some people are pleased to consider this a so-called "true fissure lode," but this fissure theory, which has been applied to the majority of lodes, is fast dying out, for, assuming a fissure to be formed for the sake of argument, any man accustomed to mining is aware that conditions which would enable the walls of that fissure to keep open until the intermediate space was filled up with mineral matter by means of water must be very rare indeed. If the country rock has simply been loosened by some dynamical agency, or even if a fault caused a fracture and displacement of the rock, any cavities due to irregularities of the walls would be filled up with rubbed off portions of the sides which have been loosened by the movement, thus packing the space between the walls like mullock in a worked-out lode. There is, then, no fissure, and the whole affair must be relegated to an exaggerated form of loosened country, the interstices of which are filled with metallic minerals brought in by solution, which cement together the brecciated and powdered rock, sometimes even replacing it. However, the Iron Blow shows no signs of this, but, on the contrary, gives every indication of belonging to that class of deposit known as segregation lodes.

The two prevailing tints that the sandstones and schists assume when not bleached are green and red. Some of the bands of rock appear to have concentrated more iron than others, at the expense of their neighbours, and at times we get very dark shades indeed; in fact, in places it is completely turned into hematite, which retains the same laminated structure that the original rock possessed. This transformation can be traced in its various stages until we come to what we may consider the adult or perfect state, as exhibited in the Iron Blow. This deposit has not been actually traced for any great length, but a similar class of stone has been found here and there on its course so far north as Mount Sedgwick, but whether these outcrops belong to the same layer of altered schist or to parallel ones will not interfere with any interpretation of their segretative character. Although the Iron Blow is composed for the most part of hematite, it is not on account of its iron that it is worked, but because it is found to be the matrix for gold. Besides hematite we also find a fair amount of baryta (specimen 558), but, curiously enough, very little quartz. The footwall is red sandstone, with quartz, and the hanging wall iron pyrites, which we will speak of later on. The general strike is nearly north and south, though locally both strike and underlay vary considerably, which is just what one would expect in a disturbed country like this. The hematite may be divided into two classes, according to its hardness, these occurring in alternate bands, the softer portion looking as if it had been loosened by friction.

We now come to a very important point. There has evidently been some great strain about here, as indicated by cracks in the sandstone and conglomerate filled with quartz, and in the limestone, occupied with calcite; also by the fact that the conglomerate found on the tops of the mountains only occur in broken patches on the saddle. I am inclined to think that as the peaks of Mounts Lyell and Owen were formed the land piled up at those points was robbed from what are now valleys, the dragged out appearance of the country leaving one under that impression. The great weight of these accumulations could not be without effect on the weakened neighbourhood, and when settling down it would have the same effect as if one were to catch hold of opposite ends of a pamphlet and press them towards each other, if I may be allowed to make use of a homely illustration. If this took place after most of the Iron Blow hematite was deposited it would account for some of it appearing to be rubbed up along the lines of least resistance, which are coincident with the planes of bedding and cleavage. It would also account for another deposit adjoining the Iron Blow, though entirely distinct from it. On the hanging wall of the hematite, as before mentioned, is a large deposit of iron pyrites (specimen 555). If we examine the structure of this we find that when undecomposed it is inclined to be schistose, and has a remark-ably close, dense texture. The boundary between this pyrites and the hematite is very sharp, which is what we would expect if due to the cause I suggest; for if such a settling down of the mountains on each side took place after the hard hematite was segregated, there being little cohesion between the hematite and the softer schists, a rupture would most easily take place between them; and the hematite, not yielding so readily to pressure exerted end on to its grain, would break up into fine particles that could occupy any loosened portion of its body; while the country rock, being more flexible, would bend rather than break, and so remain porous until infilitrated mineral solutions filled up all the spaces. The country rock about the iron pyrites deposit occurs at various angles of strike, as if wrapping round it, and the talcose schist illustrates the lenticular nature I have tried to describe on a small scale. There are no signs of sudden rupture having taken place here, but everything points to a gradual motion, which in all probability is continued at the present day. Associated with the iron pyrites we find galena; also copper pyrites, with its resulting secondary minerals—viz., malachite, azurite, cuprite, and native copper. The pyrites deposit occupies a depression or gully running 25 degrees W. of N., the hollow evidently being formed by the decomposition of the pyrites, a large portion of it being dissolved out by water in the form of sulphate of iron, while that which is oxidised into gossan remains behind as a thin capping, until denuded by atmospheric agencies. I account for the pyrites being found so near the surface by the fact that the country here is very moist, a peaty water oozing out of the button grass, the reducing action of which would retard oxidation. I also credit this water with being the agent that reduces the silver and copper which we find in the neighbourhood. The water is so darkly coloured that one almost suspects Neptune of having upset his billy-can of tea. The mass of the pyrites is very poor in gold, but the gossan (specimen 559) or iron capping in places contains a fair amount. This is only reasonable, for a ton of gossan is greater in bulk than a ton of heavier pyrites, and since a ton of gossan must be the product of decomposition of much more than a ton of pyrites, the former will naturally contain the gold concentrated in it, unless washed away mechanically, but the vesicular nature of the gossan tends to prevent that, and the sulphate of iron formed would prevent it being carried away in solution chemically. The decomposed pyrites, when placed on litmus paper and moistened, shows a distinct acid reaction, and this stone, when passed through the battery, unless well leached first and washed with alkaline water, will eat into the plates and sicken the quicksilver.

The Linda Valley Alluvial.—Most of the creeks flowing into the Linda Valley are found to be gold-bearing, as is also the alluvial of the valley itself; in fact, it was by following up the auriferous debris to the foot of the Iron Blow that this now well-known deposit was discovered, and for this reason it is generally considered that the gold found in the low country was shed from the hematite. I have carefully examined some specimens found in the alluvial, which show gold clinging to its matrix. In some instances, it is true, we find the gold attached to hematite, but we also find it sticking out of quartz. However, specimens of either sort are rare, even at the head of the gullies, the gold being, as a rule, clean and in fine grains. Now, I believe that most of the gold and alluvial found in the Linda Valley—and for the matter of that, in the neighbouring creeks also—is derived from the conglomerate which caps the mountains hereabouts. My reasons are these:—

1. There is no earthly reason why gold should not occur in the conglomerate if it were formed from a gold-bearing country, and we know the West Coast to be auriferous in several places.
2. The alluvial at the head of the Linda Valley is water-worn, and composed for the most part of quartz pebbles. In such a position the quartz, if originating from close by, could not have travelled sufficiently far to be so rounded, besides, there is not much quartz in the neighbourhood, the rocks being mostly schists and sandstones.
3. The saddle at the head of the valley connecting Mounts Owen and Lyell has very little conglomerate left on it, though traces here and there can be seen that could not possibly have fallen down from the higher mountains. If the conglomerate originally on the saddle was ruptured and loosened by dynamical strains it would be the more easily disintegrated and washed down into the valley by sub-aerial agencies than the less disturbed and more solid rock on the mountains, which would account for our not finding much conglomerate left on the saddle; then, again, the pebbles composing the gravel of the alluvial in the valley are similar in size and nature to those found in the conglomerate.
4. Boulders of conglomerate can be seen in various stages of decomposition on the sides of the mountains and in the alluvial of the valley. I have washed the product from a decomposing boulder standing isolated on its way to a gully below, and have obtained a colour of gold. On making

enquiries from local miners, two of them—Jack Fehey and Steve Karlsen—informed me that they had also obtained gold from the conglomerate wash at different times.

5. That portion of the alluvial deposit which is composed of broken up schists is very poor in gold; that made up of red standstone drift is better, while that consisting of quartz pebbles is the best, and we seldom get a "duffer" dish from it. At Karlsen's face they average about five grains per lode, while at the Linda Valley claim they estimate four and two-eleventh grains per cubic yard of dirt.

The alluvial at the head of the Linda Valley is evidently not in its original position, as can be seen by the high angle at which it is found, for the gravel, sand, clay and lignite, all of which have different angles of rest, are tilted up to such a degree that proves they could not have been deposited in such a situation. Some people shirk the necessity of explaining the problem of the Linda Valley alluvial by saying that the whole mass has been turned topsy-turvy, but there are no signs of such a general mixing up: the tilting of the deposit from the original angle of deposition is all we require to make the whole affair clear, and such a movement in the earth's crust has evidently taken place here, as the bed rock is thrown out of its normal course. The richest portions of the alluvial is found to be the top and bottom gravels, and wherever certain small, heavy white pebbles, termed locally "Sailor Jacks," occur payable gold is sure to be found. At Delany's face they have obtained good results from the former, and at Karlsen's face first-rate gold has been won from the latter. Some persons suggest that the alluvial is simply conglomerate, which has slipped down bodily from the mountains; but this can hardly be the case, as the alluvial is quite loose, while the conglomerate is very compact; besides, the former is associated with lignite which we do not find on the heights above, thus proving that the gravel was deposited gradually where now found, and that the conditions then prevailing were favourable to a dense undergrowth. The presence of occasional patches of gravel in the lignite shows that the original organic mass grew in shallow places subject to floods, and it is only in such spots that we find gold in the lignite, this going to prove that the gold in the alluvial has been deposited mechanically, and not chemically, otherwise we would expect to find it richer near the lignite, which would tend to reduce it from solution, either by virtue of the organic substance itself, or else by the sulphuretted hydrogen given off by its decomposition. Drift wood may have contributed somewhat to these lignite deposits (specimen 561), but owing to their depth I consider most of the vegetation grew *in situ*; well preserved samples of King William pine (specimen 563), and leatherwood (?) (specimen 562), can in places be picked out, and sometimes the ligneous matter has collected iron pyrites about it (specimen 560). A little cement is now and again to be met with: this in all probability was caused by pyrites being formed, an opportunity being given to the iron present to obtain sulphur from the decomposing vegetable matter, and the more heavily charged water finding its way to the comparatively stagnant bottom would cement the pebbles together. In course of time, as the deposit was drained, this pyrites would become oxidised, as we find it at the present day in Karlsen's face. Many of the streams on the Mount Lyell side of the gully have been worked for gold, until the heavy conglomerate boulders have become too numerous to remove. Besides gold, native copper is also obtained.

Mount Lyell Copper Deposits.—On tracing the alluvial copper to its source we are led to a zone of decomposed schists, forming a pug—grey, yellow, and red—some hundred feet broad. Where cut by the creeks this zone would appear to be equally productive throughout, as the tenacious clay retains most of the copper that is shed on to it; but when we clean up the bed of the watercourses we find the copper is confined to three distinct deposits, which run parallel, from two feet wide downwards. Where the pug is comparatively dry, a little below the surface, it flakes off in pieces corresponding to the cleavage plains of the shale; while the copper is found in sheets, as if occupying the joints and cleavage plains of the original rock. These sheets of metal, which at times are fairly thick, get broken up into small nuggets, shots, and spangles, which are more or less coated with the black oxide of copper. Below the native copper we come across cuprite, which occurs in beautiful crystals, mostly octahedrons, with their edges truncated by faces of the rhombic dodecahedron. No doubt the native copper has been reduced from this by the agency of the peaty waters. Still deeper than the cuprite we come across copper pyrites, which takes upon itself the same form as the rock which it impregnates; in fact, it might be termed a cupriferous schist. Instead of copper pyrites the rock may be charged in a similar manner with iron pyrites, or both. When the latter takes place the copper pyrites is found concentrated on one side and the iron pyrites on the other, and when the deposit crops out at the surface the iron pyrites resolves itself into the oxide, which is sometimes accompanied with pyrolusite.

In presenting the above remarks to the members of the Royal Society of Tasmania I have carefully weighed the ideas and opinions of others who have visited the locality I refer to.

If the glasses through which I have viewed the works of nature will not allow me to agree with the interpretation that other observers have thought fit to make, I cannot be accused of having drawn on my imagination to obtrude a pet theory upon your notice, for I have endeavoured to give a correct description of matters as they at present are found, and to point out how, by using such forces of nature as are to be seen at work at the present day, such results could be brought about, not forgetting that time is an unlimited factor of nature's that enables her to produce results slowly, yet surely, in the world's laboratory, which man, during his short space of life, can never hope to attain by the same means.

DISCUSSION.

Mr. R. M. Johnston, F.L.S., in reply to Mr. Power, said:—Of the many hypotheses advanced at different times to account primarily for the oscillations of the earth's surface it is frankly acknowledged by those who espouse one or other of them that all are open to objections of some kind.

There are three hypotheses, however, ably supported by celebrated physicists, which severally seem to find greatest favour among geologists, some espousing the one and some the rival hypotheses. These three hypotheses are mutually exclusive, and may be briefly described as—

1. The contraction or cooling globe theory.
2. The gradation or surface displacement theory.
3. Mobility of a hypothetic fluid theory.

Undoubtedly the first of these has obtained the greater acceptance among geologists, owing mainly to the able advocacy of Robert Mallet; and although it is admitted that there are serious objections to its acceptance (notably those urged by Mellard Read in his "Origin of Mountain Ranges"), it is even now variously modified, the most widely accepted among geologists as furnishing an explanation of the origin of the upheavals and subsidences of the earth's crust. Such is the opinion of Dr. Geikie in his last great work.

The rival theories, though ably supported, have even greater objections urged against them, and even one of the most formidable opponents of the contraction theory in Australasia, Professor Hutton, cannot urge that it is disproved by the demonstration of the truth of any other hypothesis. In his able address before the A.A.A.S. he states that "During the last fifty years investigation has rather been destructive than constructive, but progress has been made. Formidable obstacles have been removed . . . No doubt the outlook is still foggy, but the horizon is clearing, and we may hope that when we have fuller knowledge of the movements of the crust we shall find a clear explanation of their cause." Until that time arrives we must be content provisionally with that hypothesis which seems to account most reasonably with the greater number of known facts. The proof of a more perfect theory has not yet been made evident.

Mr. Stephens, F.G.S., said that the author of the paper under consideration had evidently paid much attention to the geological formations of a portion of the Western Country, but, in dealing with the question of the history of the mountain systems and the general physical structure of Tasmania as a whole, he appeared to have relied too much on the information supplied by maps, which is necessarily very imperfect. That the chief agency at work in producing those undulations in the primary rocks which had brought into existence many of the mountain ranges and valleys of Tasmania might be traced to secular cooling and contraction of the earth's crust there could be little doubt, and the mountain systems of later date owed their origin indirectly to the same cause, though the valleys had, for the most part, been scored out by the ordinary processes of nature working on the surface of the country. In addition to what had been said in opposition to Mr. Power's water pressure and wave theory, it might be pointed out that until portions of the crust had been raised there was no deep sea, and therefore all that the weight of the water could do, if it had any effect at all, would be to moderate the downward pressure of neighbouring mountain ranges, which are always in a condition of continuous resistance to the forces which had elevated them. With reference to Mr. Power's remarks on the conglomerates of the West Coast, there could be no doubt that, as he suggests, some of the gold in the alluvial deposits has been derived from conglomerate formed from the waste of auriferous rocks. On the other hand it has to be borne in mind that, while there are many conglomerates interstratified with the primary rocks, and likely to contain gold in cases in which the cemented materials have been derived from auriferous veinstones, there are other conglomerates of more recent date which have been derived from bands of quartz rock, quartzite, and associated schists, such as occur in great force at Mount Arrowsmith, Rocky Cape, and elsewhere, in which no gold occurs to any appreciable extent.

Mr. A. Montgomery, M.A., Government Geologist of Tasmania, regretted the absence of Mr. Power, as he would have liked to have had from him further explanation of the new theories advanced. The subject matter of the paper had a very wide scope, comprising two new theories, one of the formation of mountain chains, and a second of the origin of lodes in general, and of the Mount Lyell lode in particular.

Each of these subjects was quite large enough to require a separate paper for its elucidation. Taking the new theory of mountain elevation first, the writer referred to Mr. Johnston's supposition that many of the Tasmanian mountains are due to the shrinkage of the earth's crust, and dismissed the whole contraction theory with the cursory remark that "it has long since been proved" that that explanation was insufficient. He thought that many other geologists would agree with him (Mr. Montgomery) in denying that any such thing had long since been proved. While it might be conceded that other agencies were at work, he did not think either that many would agree that the mere presence of the ocean was the greatest. The subject of the rate of the cooling of the earth and the contraction consequent thereon was a most abstruse one, and there had been great difference of opinion expressed about it. In his mention of the higher mountain ranges, the writer left out of account the great range of mountains extending from the west of Europe to the east of Asia, and right away from any large body of water. The theory enunciated was that the mere weight of huge bodies of water pressing on the crust of the earth forced up a part of the solid crust to a higher level, and he failed to see how such a thing could happen. Water could only raise water to its own level, and could not raise a heavier substance to a higher level. The theory brought forward to account for Mounts Lyell and Owen was a curious one, and he could not see the probability of it, unless indeed the change was ascribed to some grabbing influence from outside the earth altogether, that being the only sort of force that could rake up the material from the valleys and give the country a "dragged out appearance." This and the further reference to the effect of "the great weight of these accumulations" which are no sooner raised up than they begin to settle down again, presumably directly against the force that has just raised them, led one to think that the writer must have based his theory on the supposition that the whole crust of the earth was a jelly, and he suggested that this theory might be known as the Jelly Theory, to contrast it with those generally accepted. The "homely illustration" of the pamphlet was not a good one, as the forces there applied were two lateral ones in opposite directions, while that to be illustrated by it was one force acting vertically. The writer stated that the "fissure theory, which has been applied to the majority of lodes, is fast dying out;" but his impression was that 999 out of every 1000 geologists or men connected with mining would hold that theory as one that met the greater number of cases, and assert that true lodes were almost universally found to be fissure deposits. It was a fact that such packed spaces as alluded to by the writer as being sure to occur if lodes had once been fissures were common in lodes. The difficulties of imagining that a lode fissure could be kept more or less open disappeared when it was remembered that fissures were generally uneven, and that "faulting," even to a very slight extent, would result in a series of more or less connected but irregular cavities. That many lodes were lines of fault was well known. He instanced the case of the Tasmania reef at Beaconsfield, which crosses highly inclined and distinctly bedded strata almost at right angles. In the drive on the lode westward at No. 6 level a band of black rock is found on both sides of the drive for some distance from the shaft. On one side of the lode white sandstone then makes its appearance, but the black still continues on the other side, and it is not till a further distance of 104 feet has been passed over that the white sandstone comes in on it, thus proving a throw of the beds of the country rock of that amount.

Mr. R. M. JOHNSTON expressed general approval with the criticism of Mr. Montgomery, and expressed regret that Mr. Power had not confined his attention to one of the many important subjects introduced and treated it in a more scientific manner.

MR. W. F. WARD, A.R.S.M., Government Analyst of Tasmania, said:—Mr. Power, in his notes on the Mount Lyell district, takes exception to the supposition that "many of the Tasmanian mountains are due to the shrinkage of the earth's crust," although he admits that "some slight puckering of the surface may be due to this cause." In so doing he joins issue with the great majority of geologists; for, as Geikie, in summarising the causes of upheaval and depression, says:—"With modifications, the main cause of terrestrial movements is still sought in secular cooling" (and consequent contraction). Confining our attention to the Tasmanian mountains, we may say that the highest peaks are one mile in height, the diameter of the earth 8,000 miles; the whole of this cannot therefore be called more than a "slight puckering," and it would on a 2ft. globe be represented by the thickness of thin foreign note paper. As regards the alleged inadequacy of the shrinkage to produce this comparatively slight effect, Mallet, the great authority on this branch of the subject, estimates that the *diameter* of the earth when liquid was at least 189 miles more than it is now; this implies also a shrinkage of the circumference to the extent of nearly 600 miles, so that we seem to have a very ample margin, sufficient to account not only for Tasmania's comparatively small hills, but for such enormous rock masses as the Himalayas and the Andes. Not satisfied apparently with this, however, Mr. Power invokes other agencies for the formation of mountains, and says:—

1. "The presence of the ocean is the greatest of these."

2. "We have not only the horizontal pressure of the ocean, but also the vertical pressure, and the force varies with the tides, this movement being more effective in shifting the earth's crust than a direct pressure."

3. "These ranges are like immense fossil waves rolling inland. They have a long slope on their western side, and a steep one on their eastern, just as if they had been pushed up by the sea."

The nearest approach to Mr. Power's suggestions which I have been able to find, is that of some American geologists, viz. :—"That the removal of rock by denudation from one area, and its accumulation in another affects the equilibrium of the earth's crust, and causes subsidence where deposition takes place, while the denuded area being relieved of weight rises;" this, however, has been fully met by proof that "should the removal and deposit of a few thousand feet of rock so seriously affect the equilibrium of the crust as to cause it to rise and sink in proportion, it would evince such a mobility in the earth as could not fail to manifest itself in a far more powerful way under the influence of lunar and solar attraction." If, then, the pressure of a few thousand feet of rock be incapable of raising the level of neighbouring districts to any appreciable extent, still less can the pressure of water, which has only about one-third the specific gravity of rock, be capable of slowly raising land, and sending it rolling inland "like immense fossil waves." If such were the case still greater would be the mobility and the influence of lunar and solar attraction. With regard to the Iron Blow I have found some difficulty in following Mr. Power's theory; but in view of the close neighbourhood of the iron pyrites and the hematite, of the fact that each is schistose in structure, that each contains much barytes, and that the hematite is on the footwall of the vein or deposit, I am still of the opinion expressed some time back in reply to Mr. Thureau: that the Iron Blow itself is the result of the decomposition of some of this large deposit of iron pyrites. I will only allude to the question of "fissure" veins to point out that Mr. Power quotes no authority against and suggests no substitute for "this fissure theory," as he terms it. In conclusion I can only agree with Mr. Montgomery that Mr. Power, notwithstanding his disclaimer, *has* drawn on his imagination for his theories.

NOTES ON THE MOUNT LYELL DISTRICT, TASMANIA.

By Frederick Danvers Power, F.G.S.

In reply to my critics, I must say that I was rather surprised to find myself credited with being the author of new theories. It is possible that I am the first to apply the views expressed in my paper to Tasmanian geology, but I do not pretend to be the originator of what Mr. Stephens terms the " water pressure and wave theory," or of the anti-true-fissure-lode theory, although I am prepared to uphold them until disproved, or more suitable substitutes are brought forward. In writing my paper I took for granted that the researches of such men as Dr. W. B. Carpenter, F.R.S., and S. F. Emmons were known, and therefore considered it unnecessary to back up my opinions by quoting them and others, in the same way that it would be superfluous to quote Sir Isaac Newton when mentioning gravity.

My paper was intended to give but a sketch of the geology of the Mount Lyell district. To go into detail in the present state of the country would take months of residence on the spot. In making my views public I was aware from Tasmanian literature, as also by personal conversation, that they were in opposition to those generally accepted in the island, but by bringing the results of modern research to bear on local conditions, I had hoped to direct thought out of the groove into which it had fallen. Of course I did not expect to win those who had strong views on the subject over to my side at once, but I did expect them either to give valid arguments for their side of the question, or prove mine incorrect, especially when these sentiments are due to misconceptions on their part. Mr. R. M. Johnston, though expressing his strong opinion against my views, does not offer a single argument in favour of his, or against mine, unless his statement that most geologists have adopted the contraction theory to account for mountain formations can be considered one. In replying I am debarred from elaborating on my subject, as to give it full justice would require more time than can at present be spared. I must, therefore, be content with briefly vindicating my views. The two points on which we differ are :—

1. The main cause of mountain ranges.
2. The nature of most so-called true-fissure-lodes.

In the first place I do not consider that any one theory will account for all our mountain ranges. There are several labourers at work; but my contention is that the chief of these is the pressure of the ocean waters. The other agents are not ignored; due weight has been given to the contraction of the earth, rotation of our world, denudation, sedimentation, etc. Now, I do not for one moment suppose that as our earth cooled it possessed a smooth surface. Our knowledge of molten slags, both natural and artificial, tells us that there are several cooling centres, and these would at once set up currents, which, assisted by the evolution of gases, together with the lunar and solar attractions and the rotation of the earth, would form inequalities on the surface of the globe which served as the neuclei of our continents and oceans; the world would not then be universally covered with water, as I gather Mr. Stephens premises, for elementary elevations and depressions would be formed long before the earth was cool enough to allow water to condense on its surface. Our continental plateau once formed, and water made to occupy the basins, the pressure of that water immediately begins to act and increases as its quantity was augmented. At first, no doubt, the cooling of the crust would have a greater effect in raising land than the pressure of water. Volcanic action would also be a main feature, and the tendency of our rotating earth to throw bodies towards the east, which is made so obvious by our trains and steamboats at the present day, would have a greater effect on the cooling mass than on our more solid crust; but under the conditions then existing disintegration would be more rapid than at a later period, for the rocks would not be protected by vegetation to the extent that they were later on besides, violent storms, humid atmosphere, and the large quantity of carbonic acid in the air would soon work havoc among the hills, forming much sediment which would act on the comparatively thin crust in a somewhat similar manner to which the immense weight of the ocean does now.

Dr. W. B. Carpenter, C.B., F.R.S., in a paper read before the Royal Institution of Great Britain on 23rd January, 1880, entitled "Land and Sea considered in relation to Geological Time," says that the total volume of ocean water to that of land is as 36 to 1; that most land is in the northern hemisphere and most water in the southern; that the bed of the ocean is comparatively flat, not basin shaped; it descends suddenly from a comparatively shallow bottom to a very deep one; that we must distinguish between the real and ostensible borders of the ocean basin, an elevation of 100 fathoms would generally show the real continental platforms, and would join many islands to the mainland, while if the existing land was depressed to the same amount very extensive areas of what is now dry land would be overflowed by the sea; that the movements of elevation which have occurred from time to time in various parts of the land areas of the globe have been the result of forces acting in different directions—vertical and horizontal; that extensive platforms have been raised by the former, while the latter will throw it into plications of which the elevated portions will form mountain ranges. "Also, the largest mountain chains characterise the borders of the greatest oceans, showing that the lateral pressure from the direction of the oceans was approximately proportional to the extent of the oceanic basin."

Now, the reason I chose to illustrate my views by South American and Tasmanian mountains in preference to the Urals, as Mr. Montgomery would like, was because I preferred to write of places I have visited, and not depend on maps more than necessary. And here I would remark that during my several trips to Tasmania, which have extended north, south, east and west, I have been careful to note the general direction of the main ranges, often climbing to lofty summits in order to attain my ends. However, if Mr. Montgomery prefers to discuss the Urals I am willing, and would direct his attention to the immense tract of low-lying swampy ground forming the Siberian plain to the east of the Urals, and north of the Altai, Yablonoi, and Stannovoi Mountains; also to the inland seas (Caspian Sea and Sea of Aral) and the numerous lakes forming a chain all the way north to the Gulf of Obe. Does not all this remind us that this land has at some time been submerged by the ocean, and would not require much change in its level to be flooded again? We thus see that though the Ural mountains are now away inland, they were once a coastal range, as were the easterly and westerly mountains above mentioned, which also run parallel to their former sea coast.

Tides and currents also have their effect on mountain building, not only on account of the sedimentation which they assist, but also by their own mechanical force. Thus we find the currents of the Bay of Biscay strongest opposite the Pyrenees; where the currents impinge on the Californian coast we find the Sierra Nevada; large currents curl round on the Chilian coast, and there we have the Andes; while coming nearer home we have the main current sweeping along the east coast of Victoria, close to our Australian Alps. The enormous weight of water in our oceans—36 times the volume of land—also tends to press out any inequalities that form on its bottom, and drive them onward towards dry land, where they are relieved of their pressure. To take another homely illustration, as if one were to flatten out a portion of a sheet of paper which was bulged up in a confined place. In order to bring in a flippant remark about some "Jelly Theory," Mr. Montgomery goes out of his way to apply hydraulic data to solid matter! which proves that he does not understand the situation. A solid body like rock will, as we well know, yield to pressure to a certain extent, and while it is being bent up a considerable amount of friction is developed. If the active force is removed the raised land will not fall back into its original position, for although the weight of the accumulated rock may cause a slight sinking, still the weight is not sufficient to overcome the necessary friction that the greater force was able to do; not only this, but the rock when strained will give in places, so as to ease itself, thus obtaining a state of rest, although out of its original position. We therefore see that this is not a case of balancing a dead weight, but one of steady pressure, and that it is not necessary for the rock to be so mobile as to be affected to any great degree by solar or lunar influences, as Mr. Ward suggests. It is my aim to account for the plications which we see, and know to have taken place; not to prove the earth's crust mobile like a jelly, so that it can have its form changed *ad libitum* like a rubber ball. Assuming Mr. Mallet's calculation of the 600 miles excess of original circumference to be correct, it by no means follows that all this surplus land is heaped into mountains. There are other land surfaces to be accounted for besides these, but even if these 600 miles were heaped up into our present ranges, why should these almost invariably be near past or present sea coasts, and more or less parallel to them, unless the oceans had some great influence over their structure? Again, if due to contraction, it seems rather strange that some of our highest mountains should be built up of comparatively young rocks, which could not have been deposited when most of the earth's crumplings, due to contraction, were formed, *e.g.*, the Alps, in speaking of which Professor Archibald Geikie in his text book, p. 918, writes:—"It is strange to reflect that the enduring materials out of which so many of the mountains, cliffs, and pinnacles of the Alps have been formed are of no higher geological antiquity than the London clay and other soft Eocene deposits of the South of England." I have been accused of advocating new theories contrary to well-known principles. What I have really done is to make use of nature's laws to account for observed phenomena, and not to invoke miracles to my aid. These laws of nature are in force now, and can be seen at work any day. It is no use for us to try and ignore them, for they will exist whether we like it or not.

To return to our local subject. If the ocean has not been the chief cause of the range which includes Mounts Lyell and Owen, how do my opponents account for its parallelism to the sea coast, and its steep slopes away from the sea; and why have the mountains that dragged out appearance and broken surface? Will not the phenomena observed be accounted for equally as well, if not better, by the views I have expressed than by the simple contraction theory? The pressure of water on the bottom and sides of a vessel containing it is well known; the force of the tides and currents is also appreciated. The effect of the rotation of the earth tending to throw objects towards the east is felt by our express trains and ocean boats, as is also that of winds; the shape of waves of water or sand is familiar as having the steeper side away from the force producing them; where, then, is the absurdity of accrediting the ocean with being the greatest auxiliary to the contraction of the earth in mountain building? Mounts Lyell and Owen show no signs of having been caused by simple denudation; there is no indication of there ever having been sufficient water to cut out the Linda Valley. The conglomerate capping this range was deposited before the mountains were raised, and most of the gravel in the Linda Valley is due to the weathering of the conglomerate, which was cracked and broken as the peaks raised their heads at the expense of the surrounding neighbourhood, for an inclined surface has a greater superficial area than a horizontal one.

Concerning the so-called fissure lodes, I consider the term as generally used misapplied. In our text books we find the fissure theory upheld. My old professor at the Clausthal School of Mines, Herr Dr. Bergrath v. Groddeck, states in his work on Lagerstättenlehre, "Gänge sind ausgefüllte Spalten." V. Cotta in his book writes:—"Since, according to our definition, all true lodes are aggregates of mineral matter in fissures, fissures must necessarily have first been formed and then filled. Both operations may have been independent of each other, and even when this is probably not the case still the formation of the fissure was an entirely different operation from that of their being filled with mineral matter."

Without disparaging text books, from which we learn the A B C of sciences, we really get more information from original articles and discussions on the same by men who have made researches from all points of view. Thus Mr. S. F. Emmons, one of the ablest living authorities on ore deposits, states that:—"Many deposits formerly supposed to have been deposited in open cavities have really been formed by metasomatic interchange of ore with country rock, *e.g.*, lead ore in limestone formations, Leadville, U.S.A. Very many so-called fissure veins in crystalline rocks are formed by percolation water circulating along joints and shrinkage cracks, fault planes or zones of crushed rock, which have filled the interstitial spaces and replaced the materials of the adjoining country rocks to a greater or less extent by the materials they held in solution, but are not the filling of any considerable open cavities." Mr. Richard Pearce in 1869 noted that the gangue material of the veins of Cornwall was the more or less completely altered country rock, and not foreign material brought from a distance.

Now, rocks may be so strained that faults result, but it is seldom that these faults leave open cavities that warrant the term fissure; for what would otherwise be a fissure is occupied by broken rock at the time of their formation. They are, therefore, not "true fissure lodes," such as may be seen in process of formation at the Steamboat Springs in Western Nevada, but fault lodes pure and simple, which show a brecciated structure, flucan, partings, slickensides, etc., all indicative of motion, and that usually gradual and under great pressure.

My idea of the genesis of the "Iron Blow" was that the hematite was segregated first in a layer of schist favourable for it; that after the peaks of Mts. Lyell and Owen were elevated they sank slightly, when the saddle connecting them was strained; the hematite being hard and strong was enabled to resist this force better than the weaker rock to its west, which, being loosened, located the site for the pyrites deposit. There is nothing peculiar in both the hematite and pyrites containing baryta, since they both, in my opinion, obtained their mineral from the same source, but there is no trace of any intimate connection between these two deposits, as there is a sharp line at their junction, and where we do find the pyrites decomposed it is converted into a gossan or hydrated oxide of iron, not hematite. Although the hematite deposit was not easily ruptured when pressed end on, yet the lateral thrust from the direction of the coast caused strike faults in it, as indicated by the soft rubbed-up portions where one part has slid over another.

Before condemning this opinion in an offhand manner, one should carefully weigh the pros and cons, utilising such knowledge as is possessed to enable a fair judgment to be arrived at. It is not very scientific to rest content with hypotheses because they happen to be generally accepted; we should be active in searching out the truth, and by upsetting one theory we advance one step nearer our goal, so that by opening the above questions fresh points should be gained, either by ridding ourselves of unnecessary theories or by adding fresh facts to our stock of knowledge.

TOTAL ECLIPSE OF THE MOON, 24TH MAY, 1891.

OBSERVED AT LAUNCESTON; LAT. S. 41DEG. 26MIN. 01SEC.; LONG. E. 9HRS. 48MIN. 31SEC.

BY A. B. BIGGS.

The computed times for this eclipse (disregarding the penumbra) were:—

		hrs.	min.	
First contact with shadow	...	2	29·8	A.M
Beginning of total phase	...	3	38·2	,,
End of ,, ,, ...	...	4	57·2	,,
Last contact with shadow	...	6	5·6	,,

(All local mean time.)

Unfortunately, although I had made arrangements for watching all through the duration of the eclipse, my hopes were doomed to disappointment in regard to a satisfactory view of the event. On turning out I found the whole sky overcast with clouds, the moon just peeping out now and then between the thick clouds, but always veiled more or less with haze. So transient were the views that it was some time before I could adjust the telescope to focus. Having missed the first contact with shadow, I hoped to get the beginning of total phase, but it was not until 4hrs. 0min., or 2½min. after totality had commenced, that I could get a view. There was at this time, however, a bright rim on the eastern edge of the moon, apparently scarcely obscured, the moon appearing through the thin clouds like the young crescent. Again at 4hrs. 40min., or 17½min. before the end of totality, the crescent showed itself through the haze, but shifted to the other or preceding edge. At no time during totality was the darkness at all comparable with that of a cloudy, moonless night. It was evident that even in the midst of the cone of the earth's shadow some considerable amount of the sun's light reached the moon's surface. This, of course, can only be accounted for by the refraction of the earth's atmosphere.

The moon sank out of view behind the hills with the penumbral shade conspicuous upon her western rim.

The principal points which I set out to observe, namely, the actual moments of contact, the progress of the edge of the shadow over known points of the moon's surface, the tint, the amount of darkening, etc., were almost wholly missed. No definite estimate could be made as to the tint, veiled as the moon's face was more or less all through, but no trace of coppery tint was observable, only a dark, slaty gray.

(I have since learned that in other localities favoured with a clear sky the coppery tint was conspicuous. I can only account for my missing it by its being disguised by the persistent haze.)

The following extract from *Knowledge* of 5th June, 1885, may be of some interest by way of comparison with the foregoing notes. After referring to my report of the almost total eclipse of the moon on 30th March preceding, as given in the *Launceston Examiner*, which is quoted, the editor remarks :—

"The points of interest to be noted are the opacity and slaty tint of the earth's shadow, utterly obliterating all detail on that part of the moon's surface which it covered, and the entire absence of that copper-colour which had become so familiar to observers as to be regarded by them as almost the normal hue of the earth's shadow. Now it is noteworthy that this red tint was conspicuous by its absence on the occasion of the eclipse of the moon on October 4 last, whence it would seem that the terrestrial atmospheric conditions then existing must still persist."

I was favoured by a clear sky on that occasion.

THE TRANSIT OF MERCURY, MAY 10, 1891.

Observations taken at Private Observatory, Launceston.

By A. B. Biggs.

The morning of the 10th set in with a dense fog, hiding the sun from view. However, a few minutes before the expected entrance of the planet upon the sun's face (which was set down as 9h. 42min. 15sec. local mean time), the fog partially broke, giving promise of a clear view of the transit, a promise which was by no means fulfilled. A haze again covered the sun just before the critical moment, but a careful watch was kept on that part of the sun's limb where the planet was to enter. At 9h. 43min. 56sec. a partial clearing allowed a decided notch to be seen on the sun's edge. Of course the actual contact must have taken place somewhat earlier. The internal contact, so far as could be judged, occurred at 9h. 45min. 58sec. At 9h. 51min. 17sec. the planet was estimated to be one diameter within the sun's limb. During this time a local cloud condensation was taking place which almost prevented further observation, allowing, however, an occasional glimpse between the clouds, but through a haze at best. The alternate partial brightening and overshadowing was exceedingly baffling, and as a consequence the programme which I had laid out was seriously interfered with, besides necessitating a frequent changing of the apparatus to suit the varying conditions. About noon I found the speculum so bedewed that I could not see my face in it, and I had to take it out and warm it at a neighbour's fire. I feared at first that the silver film was destroyed, as the moisture appeared to have got underneath it. However, after a careful warming it came all right, and I had no further trouble with it.

There was a clear, well-defined spot on the sun's southern hemisphere, not large, about half a minute in diameter as nearly as I could estimate, including penumbra. Between this and the planet I managed to get a few chance micrometer measures, during the intervals of comparative brightness. Whilst engaged in this a *bright spot* on the centre of the planet's disc caught my attention, apparently as if the sun were shining through a hole in the planet. A similar appearance has been noted by some observers of previous transits, but the thing seems so unaccountable and improbable that it has been attributed to optical illusion. However, I could have no doubt of its reality on this occasion. The following note is from Chambers's Astromony :—" The annulus round Mercury and the *White Spot* on Mercury during transits across the sun may now be regarded as regular concomitants of the phenomenon; but there is no agreement amongst astronomers as to the cause of these appearances. The *White Spot* has been regarded by some as indicative of volcanic action, but this seems mere fancy. Prof. Powell, with more show of reason, suggested that diffraction of light had something to do with the matter. . . . . Huggins rejects this theory. It might conceivably have its origin in the internal reflection of light in a Huygenian eye-piece." (I was using a Ramsden or *positive* eye-piece). Webbe, in his "Celestial Objects," says that it "arises probably from deception from the violent contrast and the fatigue of the eye. . . . No terrestrial analogy will explain a luminosity thus visible close to the splendour of the Sun, and it seems natural to refer it to the exhausted state of the retina" (The haze on this occasion precluded the operation both of "violent contrast and fatigue.") A possible explanation may, perhaps, be found in connection with the *atmosphere* of Mercury. The "annulus," which is usually seen around Mercury during transits, would seem to indicate the existence of an atmosphere of considerable extent. (I failed to see it on this occasion, though, I believe, so far as I can recollect, that I saw it during a former transit; but unfortunately I took no notes at that time. The haze, however, would sufficiently account for my not seeing it this time.) This body of atmosphere would act somewhat as a lens (or rather as a "spot lens") and would form a diffuse focus or image of the sun on this side the planet. We should not under ordinary circumstances see this image, because the rays would cross and pass by on each side of us, unless there were some sort of translucent *screen* on which to receive it. Such a screen might possibly consist of the meteoric matter which is supposed, or I may say is known to exist in interplanetary space, some of which reaches us in meteoric showers, and some probably mere planetary dust, occasioning the phenomenon known as the "Zodiacal Light." (Of course every particle of this would be moving in some regular orbit, in accordance with gravitation law.) This may be deemed a very fanciful theory, and possibly may excite ridicule from mature scientists; but slow progress will be made in scientific knowledge if it is to be impeded by such a fear. I have not overlooked the fact that the outer or upper stratum of Mercury's supposed atmosphere would be more rarefied and less refractive than the lower; in fact it would be unequally refractive throughout; but this, I think, would be rather in favour of the theory, inasmuch as it would lengthen the focus and bring it nearer to us.

The contacts were observed as follows:—

LAUNCESTON MEAN TIME, MAY 10.			AT INGRESS.
hrs.	min.	sec.	
9	46	56·3	Sun's limb notched. Actual contact earlier.
9	45	58	2nd or internal contact, apparent geometrical, but no appearance of solar light outside the planet.
9	47	52	In clear of sun's limb.
9	51	17	About 1 diameter clear of limb.
P.M.			AT EGRESS.
hrs.	min.	sec.	
2	31	38·25	About 1 diameter from sun's limb.
2	35	10·3	Apparent geometrical internal contact, slight ligature (?)
2	36	41·7	Apparently bisected.
2	38	40·7	Clear from sun's limb.

MICROMETER MEASURES

Between Mercury and Sun-spot.

(Mercury South. Preceding.)

Local Mean Time.			Edge of		Difference Dec.	Mer. Pre.
			Mer.	Spot.		
hrs.	min.	sec.			′ ″	sec.
0	49	8·4	N.	N.	7 27	—
0	54	38·5	N.	N.	7 40	5½
1	43	26	Centre	Centre	8 52·3	17
1	43	43	Pre.	Pre.	—	18½
1	53	20	Centre	Centre	—	19½
1	53	39·5	S.	N.	9 35·2	—
1	58	4	N.	Sun's S. limb.	1 14·6	—

The above were all the measures I had opportunity for obtaining. The chronometer readings are carefully corrected for error and rate. Mr. Sparrow, of Launceston, kindly assisted me by reading the chronometer.

OBSERVATIONS ON THE CAUSES OF ELEVATION AND SUBSIDENCE OF THE EARTH'S CRUST.

By R. M. Johnston, F.L.S.

Any enquiry into the causes which have produced, and are still producing, mountains and mountain chains cannot be satisfactorily carried out apart from the study of the causes which originally produced the grand irregularities of surface, which primarily determined the original areas of continents and oceans.

In offering any opinion of my own upon the merits or defects of the rival hypotheses assumed by so many eminent physicists to account for elevation and subsidence, I hope it will be conceded that I do so with much diffidence; for where so many eminent men fail to be satisfied with each other's views on a subject which necessarily rest so largely on arbitrary values for unknown data, it is obvious that any view which I may tend to favour must in my own mind be attended with a large measure of doubt, and on some obscure points my judgment may be suspended entirely. As a novel theory, however, has recently been referred to by Mr. F. Danvers-Power, relating to the elevation of mountain chains, it may be of some interest at this time to enter more fully into the consideration of those hypotheses of causation which have gained the most favour among physicists and geologists. Of course the correct observation of effects and the true interpretation of immediate or primary causes are very different things. The geologist, as such, is entitled to speak with authority as regards the former, but undoubtedly the correct interpretation of causation is more purely within that realm of science where the physicist, as such, has the greater claim to be heard. The geologist may best know what are the magnitude and characteristics of certain obvious changes in a given region, but his interpretation of the cause or causes at work—not so open to observation—may be liable to errors from which the knowledge of the physicist might have saved him. The apparent cause to the geologist may, to the mind of the skilled physicist, be altogether objectionable when tested by his more exact methods for determining its nature and efficiency. With some the mere parallelism of certain phenomenal features—great ocean boundaries and certain mountain chains—often appear to stand as cause and effect. This may be so; but is it true? The answer to this query will be given hereafter. As it is desirable before discussing any one view to summarise briefly the principal hypotheses which have gained the greatest acceptance among scientific observers generally. The evidences for the vast extent of movements of elevation and subsidence, past and present, are so well established that I need only confine my attention to the leading theories concerned in their causation.

Formation of Continental Areas and Ocean Basins.

Assuming that at first the earth was a heated sphere around which the existing waters of ocean seas and rivers were gathered in the form of a gaseous envelope, we are led to conclude that the radiation of heat, immediately prior to the cooling and consequent condensation of water vapours, would gradually tend to form a solid crust. The question of importance at this stage is: Was the surface matter homogeneous and smooth immediately before and after it hardened into a crust, or was it heterogeneous, containing irregularities of surface? If the former, we could not imagine how the condition for determining continental areas and oceanic basins could be initiated. We are therefore led to accept the alternative hypotheses as more in accord with known facts. But the root matter here contemplated involves the conclusion that prior to the cooling and condensation of the gaseous vapours, which eventually occupied the primæval ocean basins, the forces of themselves then at work were equal to the task of producing irregularities on the earth's surface, whether by contraction expansion, or transfer, sufficiently grand in scale to form more or less elevated continental areas in the midst of basins grand enough to receive the oceanic waters. There may not have been at this stage either deep abyssal regions on the one hand nor high mountain peaks on the other, but it is almost certain that forces then at work sufficed to produce such relative elevation and depression, as are now supposed by many to be only rendered possible by changes in the loaded surface caused by the transfer of superficial solid matter mainly through aqueous agencies. A diversified distribution of the surface magma is assumed with good reason by Mallet, J. D. Dana, Prof. Hennessy, Archdeacon Pratt, Geikie, and many other eminent physicists and geologists, as a primary condition; and this primary condition, owing to the unequal rates of cooling, and differences of density of different masses of magma, is assumed to be the initial factor in producing elevated and depressed surfaces. Dana in his last work ("Manual of Geology," 3rd edition, 1879) states:—"The fact that the continental and oceanic areas were determined in the first cooling of the globe signifies that in the cooling or the radiation of heat into space there were areas of greatest and least contraction. This difference in cooling and the resulting level of the surface must have been owing to some difference of quality or condition in the material." One quality has been brought to light by pendulum experiments in India, proving that gravity is greater at the coast stations than at the continental stations, and greater at the island stations than at the coast stations. In harmony with these observations are the opinions advanced by Archdeacon Pratt (The Figure of the Earth, 1860 ; on the Constitution of the Solid Crust of the Earth, *Nature*, 1871) who first attributed the origin of oceanic depressions and continents, and also of mountain chains, to unequal contraction in a cooling globe, but in the last refers the formation of mountains to lateral pressure, and there concludes "That the crust beneath the ocean is of greater density than the average portions of the surface—that is, where the contraction was greatest the density of the rich material below is greatest and proportionately so." Mr. George H. Darwin, in investigating the bodily tides of viscous spheroids (Phil. Trans., 1879), among other important matter also points out that under the combined influence of rotation and the moon's attraction the polar regions tend to outstrip the equator, and to acquire a consequent and slow screwing motion from east to west ; and Dr. Geikie, who has certain objections to offer, admits in respect of Darwin's theory, that it is conceivable in earlier conditions of the planet, that this screwing motion may have had some influence in determining the surface features of the planet. In a body not perfectly homogeneous it might originate wrinkles at the surface running perpendicular to the direction of greatest pressure. "According to this theory the highest elevations should be equatorial, and should have a general north and south bend, while in the northern hemisphere the main direction of the masses of land should bend round towards the north-east, and in the opposite hemisphere towards the south-east." While there are many difficulties standing in the way of the full acceptance of Mr. Darwin's theory, Dr. Geikie still thinks "It is well worth consideration whether the cause suggested by Mr. Darwin may not have given their *initial trend* to the masses of land, *so that any subsequent wrinkling of the terrestrial surface due to any other cause would be apt to take place along the original lines.*" I might also add that to such influence in some measure might be still ascribed those remarkable lines of weakness in both hemispheres, which mark the course of volcanic action, and which, in a large measure, *may determine the curves of certain coastal mountain chains.* Apart, however, from the universality or antiquity of alleged causes advanced as the principal factors in mountain making (I here use the word mountain-making in preference to mountain-building, for the latter term is more appropriately restricted to peaks and cones built up by volcanic agency), it must be confessed that whatever mountain chains were formed in the earliest geological periods they have been largely obliterated in later times. This is becoming more evident as geological observation extends; for it is now well established that in Asia, South America, North America, and in Europe, the great era of mountain making was during the Tertiary period, and even in Australasia it is probable that its principal Alpine ranges were inconspicuous ridges during the early part of the Mesozoic period. Still the intensity and universality of the elevations then produced demand a corresponding universality of cause, or causes. A considerable number of hypotheses at different times have been proposed by eminent investigators, all of which commend themselves in some particulars, but all of which, taken separately, involve difficulties which at present appear to be insurmountable. Perhaps the truth lies in several, and, *at least*, does not demand the rejection of one cause because another seems to answer all the conditions when confined to a particular case. It appears to me, however, that there is one which may be well considered as the governing or mother cause to two or three of the rival hypothetical causes; the latter being consequents standing in the relation of child to parent rather than absolutely independent causes. The parent cause, in my opinion, appears to be the Contraction Theory so intimately associated latterly with the name of Robert Mallet.

Contraction Theory.

If our planet has been steadily losing heat by radiation into space a corresponding diminution in its volume must also be looked for; for cooling is implied in contraction. A succinct account of the effects which Mr. Robert Mallet demonstrated might be expected from the secular cooling of our globe is given as follows by Dr. Geikie:—"According to Mallet the diameter of the earth is less by 189 miles since the time when the planet was a mass of liquid. But the contraction has not manifested itself uniformly over the whole surface of the planet. The crust varies much in structure, in thermal resistance, and in the position of its isogeothermal lines. As the hotter nucleus contracts more rapidly by cooling than the cooled and hardened crust the latter must sink down by its own weight, and in so doing requires to accommodate itself to a continually diminishing diameter. The descent of the crust gives rise to enormous tangential pressures. The rocks are crushed, crumpled, and broken in many places. Subsidence must have been the general rule, but every subsidence would, doubtless, be accompanied with upheavals of a more limited kind. The direction of these upheaved tracts, whether determined, as Mr. Darwin suggests, by the effects of internal distortion or by some original features in the structure of the crust, would be apt to be linear. The lines, once taken as lines of weakness or relief from the immense strain, would probably be made use of again and again at successive paroxysms or more tranquil periods of contraction. Mr. Mallet has ingeniously connected these movements with the linear direction of mountain chains, volcanic vents, and earthquake shocks. If the initial trend to the land masses were given as hypothetically stated by Mr. Darwin we may conceive that after the outer parts of the globe had attained a considerable rigidity, and could then be only slightly influnced by internal distortion, the effects of continued secular action would be seen in the intermittent subsidence of oceanic basins already existing, and in the successive crumpling and elevation of the intervening stiffened ridges." To overcome the difficulties which arise out of one or other of these hypotheses various modifications of the contraction theory have been generally adopted, the greater number still adhering to the view that the *main cause* of terrestrial movements must be sought in secular contraction. The theory which seems to be most favoured next in importance to the Contraction Theory is:—

> The alleged expansion and contraction of the underlayers resulting from a rise or fall of temperature caused by the loading or unloading of the areas affected.

Great uncertainty still exists as regards the present condition of the earth's interior; some conceiving it to have a fluid or viscous central core; some again incline to the view that a viscous intermediate layer separates the solid crust from the solid nucleus; while others have given reasons for the view that on the whole it has now attained a rigidity equal to that of glass or steel.

EXPANSION AND CONTRACTION OF THE UNDERLAYERS RESULTING FROM A RISE OR FALL OF TEMPERATURE.

Babbage seems to have been the first to suggest this theory. In his memoir (1834) on the Temple of Serapis, "Besides recognising the relations of isothermal planes, and the effect upon them by the surface changes, whether removals of rock material, or accumulation, the memoir accounts for changes of level caused through the expansion or contraction caused by changes in the subterranean heat or in the position of these isogeothermal planes." Mr. Mellard Reade has lately, "Origin of Mountain Ranges," strongly insisted on this factor as an important if not the main factor in producing subsidence and elevation. Similar views have also been strongly advocated by some of the American geologists who have explored the western territories of America. These have pointed "in proof of its truth to evidence of continuous subsidence in tracts where there was prolonged deposition and of the uprise and curvature of originally horizontal strata over mountain ranges like the Uintah Mountains in Wyoming and Utah, which have for a long time been out of water." Dr. Geikie, in commenting upon this theory, admits that in so far as the internal structure of rocks may be modified by such progressive increase of temperature as would arise from superficial deposit, the cause of change must have a place in geological dynamics, but he cannot allow that the removal and deposit of a few thousand feet of rock should exert such an influence as to affect the equilibrium of the crust; for to admit this, "would evince such mobility in the earth as could not fail to manifest itself in a far more powerful way under lunar and solar attraction." He, however, goes on to say "that there has always been the closest relation between upheaval and denudation on the one hand and subsidence and deposition on the other, is undoubtedly true." But he adds the significant words that "denudation *has been one of the consequences* of upheaval, and deposition has only been kept up by continual subsidence." Certain questions bearing upon the permanence of continental areas and great oceanic basins are involved to a great extent in the views under discussion, and may have led to the adoption of restricted views as to fundamental laws of causation.

Prof. C. Loyd Morgan in a recent article on elevation and subsidence (Geol. Mag., July 1888) in criticising the views of Mr. Mellard Reade and others as regards the effects alleged to arise out of the transfer of sediments suggests other ways in which the loading and unloading of the earth's crust may indirectly bring about subsidence and elevation, and at the same time ingeniously advances reasons in favour of the existence of an underlying liquid or viscous substratum. Accordingly, without committing himself to the acceptance of the theory held by those who attribute subsidence to mere weight he suggests to the upholders of that theory that the added weight of the sediment above would entail on this hypothesis an added weight below—that is if we suppose that the solidified rock adheres to the lower surface of the crust region. In a region undergoing denudation, on the other hand, the lightening of the load would entail the melting of some of the solidified or crystallised magma, assuming with Mr. Mellard Reade and Mr. Davison, that owing to the cooling and contraction of the earth's crust there is at some depth beneath the surface a level of no stress, where there is neither lateral compression nor extension, though the rocks are subject to the vertical pressure of the overload. He then suggests that throughout the zone of maximum tension due to circumferential contraction the rocks may be rendered fluid by relief of pressure. Such melting, he adds, would be accompanied by expansion manifesting itself at the surface by an uplift. By the expansion of the melting underlayers tensile stress in the overlying strata would be called into play, and this would throw those strata into a state of tensile strain, thus giving origin to normal faults to the gradual gaping of mineral veins and dykes, into which the molten matter would be injected by the expansive force. Without denying, therefore, the influence of secular refrigeration, he suggests that we have on this hypothesis an efficient primary cause of volcanic eruptions. Commenting upon the differences of opinion as to the level of the stress he points out that according to Mr. Davison it lies five miles deep; according to Mr. Mellard Reade it is taken at one mile; while Mr. O. Fisher would reduce it to less than a mile; and accordingly Prof. Morgan urges caution as to the use of precise imposing mathematical calculations based upon arbitrarily selected data where it concerns problems "concerning which the most noteworthy feature is our profound ignorance."

MR. DANVERS-POWER'S REFERENCE TO THE SUPPOSED INFLUENCE OF THE PRESSURE OF OCEAN WATERS UPON THE FORMATION OF PARALLEL MOUNTAIN CHAINS.

In regard to this hypothesis I must confess that to me it seems to be a most extraordinary one. Mr. Danvers-Power does not give us the slightest indication to help us to conceive how, of all agencies that may be concerned in the dynamics of mountain making, "the presence of the ocean is the greatest." It is hardly necessary to remind us how great is the influence of water in the work of denudation, and in the redistribution of wasted rock sediments over lower levels of sea and land; but how the gravity or even surface movements of ocean waters extending over wide ocean areas can concentrate their force of gravity or pressure laterally by thrust upon the margins of continents is a nice puzzle to any physicist. Surely it must have occurred to Mr. Power that the vertical radii of a column of rock is from 2½ to 3.4 times the specific gravity of an equal column of water; that the radii of a continental area are much greater than the radii over the more depressed ocean areas, capped as they are with the light element water; that the pressure of a fluid upon any of its limiting surfaces is altogether independent of its quantity (known as the *hydrostatic paradox)*; that is—the total pressure of water, (still) against and perpendicular to any surface is equal to the weight of a uniform column of water, the area of whose cross-section parallel to its base is everywhere equal to the area of the surface pressed; and whose height is equal to the *vertical* depth of the *centre* of gravity of the surface pressed below the surface of the water. It is a natural mistake among young students of hydrostatics to fall into the error that the magnitude of the horizontal expanse of any body of water intensifies the pressure upon its limiting surfaces; but the determining factors of pressure given hereinbefore shows that the horizontal pressure upon the vertical face of any wall or embankment, say 100 feet deep and 100 feet wide, would just be the same if the width were extended 1,000 miles or to any conceivable extent *so long as the area pressed* and the vertical depth of its centre of gravity below the level surface remain unchanged. That Mr. Danvers-Power has fallen into this elementary error as regards the lateral pressure of oceanic water is beyond doubt, for there is no escape on the plea of misconception or ambiguity in the following statement intended by him to show that greatness of *expanse* or area of ocean intensifies lateral pressure upon coast lines. Thus in accounting for the parallelism of high chains of mountains with great ocean basins, he states—" We almost invariably find that the highest mountain ranges have been developed more or less parallel to the past or present sea-coasts that are washed by the largest bodies of water. Thus, in South America, we find the Andes on the west higher than the mountains of the east coast; also that the western shores of America are washed by the Pacific Ocean, which is greater in area and depth than the Atlantic. Coming nearer home for an example we find the highest Australian Mountains on the East Coast, the Pacific again being larger than the Indian Ocean. In Tasmania, however, matters are somewhat different, for there is an unbroken stretch of water from its West Coast to South America; while on its East Coast some twenty degrees distant *New Zealand acts as a breakwater*, and braces up the ocean so to speak, relieving the East Coast of Tasmania of much pressure from the main body of water;" and further on he again states:—" We not only have the horizontal pressure *of the ocean*, but also the vertical pressure." In his reference to Tasmania he is extremely unfortunate, for there is nothing more probable than that Tasmania, as *a whole*, forms but a southern prolongation of the Eastern Alps of Australia, and the causes which led to the original determination of the main chain operated in the formation of its southern extremity. And again he does not recognise the fact that the highest Alps of the Australasian region run along the *Western Coast* of New Zealand in a line north easterly and parallel to the south eastern coast of the mainland of Australia, and are not found on the eastern border, which is washed by the full sweep of the South Pacific Ocean, to whose influence he ascribes the smaller Alps of the Eastern border of the mainland of

Australia. But apart from such consideration it is clear from his observations that he regards the largeness of the *ocean area* as a factor in intensifying the horizontal pressure of its waters, and thus runs counter to one of the elementary laws determining the horizontal pressure of fluids. When we consider this limitation we can perceive how insignificant must be the influence of lateral pressure of ocean waters upon coast lines when compared with the enormous force required to bulge the solid earth into mountain chains. Again, as regards the energy of water set in motion by winds, and the currents induced by lunar and solar action, it is clear that the amount and direction of such forces do not harmonise so closely with the direction of the lateral thrusts, which have produced those remarkable chains of mountains like the Andes, that their influence *should be added* to the supposed influence of the pressure of oceanic waters already referred to. As regards the reference to Dr. Carpenter I cannot see that the quotations given have any bearing upon the pressure exerted by oceanic *waters* on their land border. Indeed, Mr. Danvers-Power does not seem to be aware that the last quotation referred by him to Dr. Carpenter is nearly exactly word for word identical with, and, no doubt, adopted from Dana's theory of the elevation of mountain chains parallel to the great oceans. (Dana in p. 828, Manual of Geology, 3rd edition, 1879). Dana here affirms (prior to Dr. Carpenter), "The fact that the largest and loftiest mountain chains, greatest volcanoes, and other results of uplifting and disrupting force characterise the borders of the largest oceans, shows that the shoving *action from the direction* of the oceans was approximately proportional to the ocean *basins*; but Dana clearly explains that this shoving motion is not due to the superficial waters contained in these basins, but that " the landward action of the force seems to be a necessary consequence of the fact that the *crust* over the oceanic areas was, and is abruptly *depressed below* the level of the continental, so that the lateral pressure from its direction would have *the advantage of leverage beneath the continental crust*, or rather would have acted *obliquely* upward against it." There is a fundamental difference here between the lateral pressure of ocean water, and the oblique upward lateral thrust of the ocean bed, and Dr. Carpenter's quotation is in entire harmony with Dana's theory, and gives no support to Mr. Danvers-Power's *ocean water theory.* The idea originated by Archdeacon Pratt that the rocky material under the ocean is more dense than that under continents has also influenced Dana in ascribing greater stiffness or rigidity to oceanic areas, and so determining in some measure the peculiarities connected with mountain chains running parallel to the great ocean. At any rate their arguments in no way support the novel theory of Mr. Danvers-Power. As regards the parallelism observed between oceans and mountain chains, I have only to observe that such parallelism need not be interpreted as cause and effect. Although, if we assume original flexures on Darwin's theory, trending in the present direction of these chains it is certain that if the continental areas so sculptured were slowly submerged in the direction of the great ocean areas, that the basal contour lines of height would certainly form the barriers to the ocean basins, and these of necessity would produce the parallelism referred to—*i.e.*, the downward sinking of the land would better explain the parallelism observed than the reference to pressure from the direction of the ocean, whether from its bed or its superficial waters. I think, therefore, that good reasons have been given for rejecting the theory put forward by Mr Danvers-Power.

A NOTE ON THE ARITHMOMETER AS AN AID TO ACTUARIAL WORK.

By F. J. Jacobs, A.I.A.

The arithmometer is peculiarly adapted for working out the chief details of a classified valuation of a life assurance office. The process is a simple one, being merely a series of multiplications, and the work can be carried out very rapidly by an ordinary computer. The results worked out and set down by one computer can be checked and verified by a second computer using a different machine, while any errors in the original work may be investigated by a third worker, using a third machine. By this means complete accuracy is ensured, and the use of the different machines avoids any possibility of error should either of them happen to fall out of order and become liable to repeat its own mistakes.

Several able expositions of the uses of the arithmometer have been published, and amongst them may be cited papers by Major-General Hannyngton and Mr. Peter Gray in the 16th and 17th volumes respectively of the *Journal of the Institute of Actuaries*. At the beginning of his paper Mr. Gray remarks:—" It is usual to describe the arithmometer as a machine which enables a person, however unskilled himself, to perform the operations of multiplication and division with facility, rapidity, and unfailing accuracy. This, as a description, is correct as far as it goes, but as an enumeration of the properties of the machine it is inadequate and defective. It entirely omits that property which forms its special adaptation to our purpose, and in default of which its utility would be comparatively limited. Besides the facilitation of the operations named the machine will also in forming the product of two given numbers either add that product to or subtract it from another given number according to the pleasure of the operator." Mr. Gray then proceeds to give illustrations of the adaptability of the machine for the construction of tables, and amongst the acturial problems he investigates are:—

1. To form a table of assurances from a corresponding table of annuities.
2. To construct a table of policy values from a given table of annuities.
3. To construct commutation columns from any given mortality table.

Amongst modern tables constructed by the calculating machine must be mentioned those computed by Mr. David Carment, F.I.A. A copy of the work is submitted for inspection. These tables show the value at the end of any number of years of an endowment assurance for £100, payable at different ages and under different rates of interest, according to the "Institute of Actuaries H.M. Tables of Mortality." In constructing these tables Mr. Carment utilised a modification of Gray's continuous method, and, speaking briefly, the work consisted in placing upon the machine a constant (the reciprocal of a certain function) and multiplying this by the successive differences of another function, the results being, of course, taken down after each operation. The process of the work is lucidly explained by Mr. Carment in the preface to his tables. It must be noted that the multiplication referred to is performed continuously, and that each step is performed *without* erasing the result attained by the preceding operation. The correctness of each column of results is proved by a very simple calculation. The whole of the tables were calculated by means of the arithmometer, with the exception of a small portion at the end, which. owing to the partial breakdown of the author's machine, were done by another process; the book is an interesting example of the work that can be performed by the calculating machine.

I have frequently had occasion to use the arithmometer in the computation of different results, and have found it a most correct and expeditious means of performing work that might, under ordinary circumstances require a tedious mental application. The arithmometer now exhibited by Mr. Johnston is, as he has stated, of the latest pattern, and combines the original model with a stability and perfection of construction that should reduce to a minimum the possibility of errors arising from imperfect or impaired machinery.

HYPNOTISM AND ALLIED PHENOMENA.

By Walter Spencer, M.D., M.R.C.S., Etc.

In judging newly observed phenomena for the purpose of reconnoitering the field for further exploration it is not easy to steer a medium course between the caution which demands too much and the receptivity which is satisfied with too little investigation. The former borders upon obstructiveness, the latter merges into credulity. In the first part of this paper I shall state ascertained facts, after which I shall have to lay bare the dilemma between necessary caution and regrettable credulity.

In 1843 Dr. James Braid, of Manchester, found that he was able in certain patients to induce an artificial sleep, accompanied by insensibility to pain. He made use of the power more for the relief of pre-existing pain than as a help in surgery, but under his auspices the eminent dentist, Sir J. Tomes, was surprised to find that he could extract several teeth without his patients having any consciousness of the operation. Dr. Braid used to rivet the attention and eyes of the patient upon something shining placed close at hand so that it was a strain to look at. He considered that fatigue of the ocular muscles was a chief factor in inducing the required condition. The new process was christened Braidism, but was not adopted to any extent in England, the triumph of the easy anæsthetics, chloroform and ether, seeming to render its practice unnecessary. In France, however, two great schools devoted to the subject (which Braid called hypnotism) have conducted interesting experiments for many years, under Professor Charcot, at the Salpétrière, Paris, and Profesor Liébault at Nancy, the latter becoming known to fame as the originator of "hypnotic suggestion." Charcot is one of our foremost authorities on the management of hysteria, and has pursued the enquiry into hypnotism chiefly with reference to that abnormality of nerve function. We shall see, however, that the popular impression as to hysterical subjects being most predisposed to hypnotic influences is by no means proved.

At a general meeting of the British Medical Association held in August, 1889, a special section was devoted to hypnotism. It resulted in the expression of divergent opinions, in which, broadly speaking, French physicians maintained the affirmative and English physicians the negative view of the value of the study. Amongst the speakers was Dr. Voisin, of the Salpétrière, who stated that he had by this means cured persons suffering from hallucinations and from sensory disturbances. Suicidal ideas and mania had disappeared under the use of the method. He had succeeded in curing dipsomania and morphinomania, as well as obstinate cases of juvenile depravity. Some of his patients had been put to sleep for from six to eight days. He had been able to transform his patients' habits of thought, and had brought them to love the good, whereas they before had only loved the evil. Dr. Yellowlees expressed amazement at these assertions, and remained incredulous, but Dr. Tuckey confirmed them from his experience of the practice of Dr. Liébault, who had successfully treated dipsomania and neuralgia. He cited the institution at Croydon, conducted by the Rev. Arthur Tooth, for the treatment of inebriates, where hypnotism was employed with much success, but he had been disappointed in the results of its application to the cure of melancholia, only 6 per cent. of the patients having proved susceptible. In the majority of cases it was not true that a patient after hynotism remained a mere tool in the hands of the operator. Dr. Ireland remarked that Braid had made claims as startling as those of Dr. Voisin. In one instance which he had published a flow of milk had been brought into one breast of a woman, while the other remained flaccid. Dr. Robertson said he had visited the Salpétrière Asylum, and could corroborate Dr. Voisin. Dr. Percy Smith, of Bethlem Hospital, concurred with Dr. Yellowlees. He had made trial of the method with the assistance of a professional hypnotiser, and the results had been almost entirely negative; in fact, he had not been able to get insane patients into the hypnotic state. It seemed impossible to secure the necessary attention. This view of the question was confirmed by Dr. Goodall and others. Dr. Turnbull Smith asserted that in his experience nineteen patients out of every twenty had proved to be susceptible, but in some cases only after repeated attempts. Exhaustion of the optic nerve by means of staring had been found most efficacious.

This divergence of opinion extends to the methods recommended. Dr. Forel, of Zurich, states that he had noticed very ill-effects from using what he terms "the old bad method of fixation of vision."

Last year the attention of the medical profession in England was aroused by the report of a meeting held at Leeds, on March 28, of some sixty medical men and dentists assembled, at the invitation of Messrs. Carter Brothers, to witness a series of operations under the hypnotic influence of Dr. Milne Bramwell.

The first case was hypnotised at a word, and three teeth extracted without the operation having been felt.

The next case was that of a servant girl, M. A. W., aged 19, on whom, under the hypnotic influence induced by Dr.

Bramwell, Mr. Hewetson had a fortnight previously opened and scraped freely, without knowledge or pain, a large lachrymal abscess, extending into the cheek. Furthermore, the dressing had been daily performed and the cavity freely syringed under hypnotic anæsthesia, the "healing suggestions" being daily given to the patient, to which Dr. Bramwell in a great measure attributes the very rapid healing. She was put to sleep by the following letter from Dr. Bramwell, addressed to Mr. Turner:—

"Burlington Crescent, Goole, Yorks. Dear Mr. Turner,—I send you a patient with enclosed order. When you give it to her she will fall asleep at once and obey your commands.—J. MILNE BRAMWELL.

"Order.—Go to sleep at once, by order of Dr. Bramwell, and obey Mr. Turner's commands.—J. MILNE BRAMWELL."

This experiment answered perfectly. Sleep was induced at once by reading the note, and was so profound that, at the end of a lengthy operation, in which sixteen stumps were removed, she awoke smiling, and insisted that she had felt no pain, and, what was remarkable, there was no pain in her mouth. She was found after some time, when unobserved, reading the *Graphic* in the waiting-room as if nothing had happened. During the whole time she did everything which Mr. Turner suggested, but it was observed that there was a diminished flow of saliva and that the corneal reflexes were absent, the breathing more noisy than ordinarily, and the pulse slower.

Dr. Bramwell took occasion to explain that the next case, a boy aged 8, was a severe test, and would probably not succeed, partly because the patient was so young, and chiefly because he had not attempted to produce hypnotic anæsthesia earlier than two days before. He also explained that patients require training in this form of anæsthesia, the time of training, or preparation, varying with each individual. However, he was so far hypnotised that he allowed Mr. Mayo Robson to operate on the great toe, removing a bony growth and part of the first phalanx, with the result that he appeared to know very little of what had been done.

It was necessary in this case for Dr. Bramwell to repeat the hypnotic suggestions. Dr. Bramwell remarked that he wished to show a case that was less likely to be perfectly successful than the others, so as to enable those present to see the difficult as well as the apparently easy straightforward cases; "in fact," as he said, "to show his work in the rough."

The next case was a girl of 15, highly sensitive, requiring the removal of enlarged tonsils. At the request of Dr. Bramwell Mr. Hewetson was enabled in the hypnotic state to extract each tonsil with ease, the girl, by suggestion of the hypnotiser, obeying every request of the operator, though in a state of perfect anæsthesia. In the same way Mr. Hewetson removed a cyst of the size of a horse bean from the side of the nose of a young woman who was perfectly anæsthetised and breathing deeply, and who, on coming round by order, protested " that the operation had not been commenced."

Mr. Turner then extracted two teeth from a man with equal success, after which Dr. Bramwell explained how his patients had been completely cured of drunkenness by hypnotic suggestion. To prove this to those present, and to show the interesting psychological results, the man was hypnotised, and in that state he was shown a glass of water; he was told by Dr. Bramwell that it was " bad beer." He was then told to awake, and the glass of water offered him by Dr. Bramwell. He put it to his lips, and at once spat out the " offensive liquid."

Mr. T. S. Carter next extracted a very difficult impacted stump from a railway navvy as successfully as the previous case. Dr. Bramwell described how this man had been completely cured of very obstinate facial neuralgia by hypnotism, which had been produced by working in a wet cutting. On the third day of hypnotism the neuralgia had entirely disappeared (now some weeks ago) and had not returned. The man had obtained refreshing hypnotic sleep at nights, being put to sleep by his daughter through a note from Dr. Bramwell or by a telegram, both methods succeeding perfectly.

The facts as recorded may be taken as establishing the existence of hypnotism and of hypnotic suggestion. The divergent opinions of practitioners are, however, very hard to reconcile.

Dr. Moll denies that any hysteria or weakness of any kind in the physique of a patient leads to his being more readily hypnotised. Professor Wetterstrand says it depends more on character than temperament, and that the most impressionable ages are from 3 to 15, fit subjects after the age of 30 being rare. Dr. Sherman Bigg maintains that no man of average capacity can be hypnotised against his will, and that even when hypnotised he can restrict the hypnotiser's influence. Professor Bernheim believes that the suggested effects exist only in the idea of the patient; that if it is suggested that he cannot see a certain object he continues to see it, but imagines he does not, and acts accordingly. He is of opinion that the number of persons who can be readily influenced is very limited, and yet at a demonstration given in Paris he elicited confessions of imaginary crimes, weeping, laughter, dumbness and powerlessness at will from persons who had not been previously hypnotised, and who, to all appearance, were in full possession of their senses, playing on their nervous system as upon an instrument.

At a meeting of the Medical Institute of Liverpool Dr. Grossmann reported he had seen Dr. Bernheim hypnotise one patient after another in his hospital wards until not one remained awake; also that a blister was produced by the application of a postage stamp, which, it was suggested, would act as a vesicant.

Dr. Liébault asserts that concentration of a patient's mind on the idea of sleep will induce sleep by leading to forgetfulness of the outer world and to a state in which suggestions are readily adopted and acted on, the hypnotic differing from the ordinary sleep in that there exists a relation between the sleeper and the operator. Dr. Forel considers that hypnotism is a state of sleep artificially induced, during which the course of the patient's dreams can be influenced. It depends, firstly, on the power of falling asleep easily; and, secondly, on willingness to submit to the operator's will, the disappearance of nervous symptoms being explained in the same way as the sudden cessation of toothache or sciatica after a shock. He could induce sleep readily in fifty patients out of seventy-two.

The school of Nancy teaches that the lethargic, cataleptic, and somnambulic stages of hypnotism can be produced at any time by the suggestion of the operator, suggestion being the key to all the phenomena. The psycological stages of hypnotism, although they may be divided into three—lethargy, catalepsy, and somnambulism—are variously classified by different observers.

According to Wetterstrand they reveal:—

1. Sleepiness and sense of weight.
2. Inability to move a muscle.
3. Unconscious automatic movements.
4. Consciousness only of the operator and obedience to his will.
5. Somnambulism and complete insensibility.

According to Haidenheim:—

1. Imitative movements.
2. Exaltation of special senses.
3. Illusions and hallucinations.
4. Loss of pain and of sensation.
5. Tonic spasms.
6. Spasms of the ocular muscles, dilatation of the pupils, accelerated pulse and respiration.

According to Liébault:—

1. Torpor of limbs and somnolence, the will power being still available if needed.
2. Retention by the limbs of any strained posture, the patient still remaining conscious.

3. Perception of the operator only.
4. Advanced automatism.
5. Oblivion.
6. Submission to the operator's suggestions.
7. Susceptibility to post-hypnotic influence.

The methods of operators vary. Some employ slow passes made at a short distance, some stroking, some ocular fixation, some orbital compression, some any weak, continuous, and prolonged stimulation of the nerves of sight, touch, or hearing, some verbal suggestion alone. Haidenheim invites the patient to sit down, tells him that sleep will cure him, urges him to concentrate his mind entirely on sleep, looking at him very keenly all the while; tells him he will feel weight in his eyeballs and limbs, and gradually be unable to move. And when sleep has been induced the desired instructions are suggested to him.

Liébault begins by reassuring the patient, telling him to banish all fear and extraneous thoughts, to concentrate his mind upon following the operator's words and suggestions. He then suggests the stages of sleep one by one as follow:—Your eyelids are becoming heavy; you can hardly keep them open. My voice sounds more and more distant. Your sight grows dim; objects appear indistinct. Numbness is creeping over your limbs. You cannot keep awake; your eyes are shut. (Here he closes the patient's eyes.) You are fast asleep. He then rubs any affected parts, suggesting that pain is to pass away; that disturbed functions are to be restored to normal action, and so on, concluding with the suggestion that no hypnotiser's influence except his own shall be obeyed. He arouses the patient by a word or a few passes across the face. The foregoing cannot fail to remind us of the reported procedure of American faith healers.

Charcot induces the stage of lethargy either by compressing the eyelids or by causing the patient to gaze fixedly at a bright point; that of catalepsy by lifting the eyelids during the preceding stage or by a sudden flash or shock, and that of somnambulism by gently rubbing the top of the patient's head.

Variety of opinion and of practice is accompanied by variety of theory. Among theorists Mr. Julian Hawthorne regards hypnotism as akin to the mental exaltation of the poet and the orator. Dr. Regnier finds in it nothing of this sort, not even the ecstasy of the martyr or religious devotee. The practice is not devoid of danger. Dr. Julius Lolow records how an amateur at a friend's house volunteered to hypnotise another visitor, and after two trials succeeded so well that the subject became extremely excited, lost the power of speech, and then passed into the condition of catalepsy. Subsequently he had severe convulsions. He had been hypnotised by being made to look at a diamond ring, and afterwards the sight of anything glittering threw him into a state of violent excitement. The floor of the room in which the physician found him was covered with cushions, as he frequently threw himself from the sofa on to the floor. He performed various odd automatic movements, slept only in snatches, awaking in nightmare, and, in fact, was in a condition to which the French physicians would probably apply the term grave hysteria with maniacal excitement. He was treated with full doses of sedative drugs, but did not at first show any signs of amendment. After ten days the convulsive attacks were replaced by periods during which he sang persistently. He would sing over song after song, apparently every song he knew, and as long as one song remained unsung nothing would stop him. After about a fortnight of this sort of thing he had an attack of fever, followed by copious perspiration and asthma. A few days later he had another feverish attack, again followed by perspiration, after which he declared himself quite well. From first to last he was seriously ill for three weeks. The cause of the fever is not very clear. His physician believed it was probably due to inflammation of the anterior part of the brain. Instances similar to this have not been infrequent. It has therefore been resolved by the committee of the British Medical Association that public exhibitions of hypnotism are dangerous, and should be prohibited, a prohibition which has already become law in Belgium, France, Russia, and Portugal, the only result to State medicine of the study of hypnotism thus far.

I have hitherto been dealing with facts which appear to obtain general acceptance. Allegations are made as to further phenomena which require a good deal more patience in investigation. The power of raising a blister by means of an inert postage stamp or of causing a patient to obey commands given by letter shortly after leaving the hypnotiser is transcended by the alleged power to compel obedience after a considerable lapse of time. Induction of a condition of double consciousness, alternation of two separate mental states, lasting each for considerable periods, in which two different personalities, unconscious of each other, are revealed, is more extraordinary still, while the marvels of somnambulism, the display of faculties independent of the senses, reading with bandaged eyes, and description of simultaneous remote occurrences rival old legends of possession.

It will be useful to give you a brief sketch of the observations which preceded hypnotism. When we remember that this is not a new science, but only a new name, and that it is the habit of mankind to re-christen and adopt to-day many things which it rejected and reprobated yesterday, it should make us tolerant of other allegations the truth of which is not yet established.

About 1670 a gentleman named Valentine Greatrex, the "stroker" who had acquired repute as a healer of disease by what was probably a combination of hypnotism and massage, was actually commissioned by King Charles II. to exercise his skill during the prevalence of certain epidemic diseases. In 1772 Anthony Mesmer, M.D., of Berlin, began to investigate the influence of magnetism, and is said to have effected many cures. On propounding his theory of animal magnetism he provoked a storm of envy and malice which overwhelmed him, many of his patients, even after their transports of gratitude had cooled, being so swayed by popular prejudice as to aver that their cures must have been effected by chance. He was ejected from Germany, and no University deigned to give his claims fair trial, though they have come down to us under his name as "Mesmerism."

Surgeons of reputation, Recamier in 1829, Cloquet, and others, made occasional use of the method. In 1837 Dr. Elliotson established in London a Mesmeric Hospital, where he performed serious operations painlessly, but as he persisted in proclaiming the superinduced phenomena of "clairvoyance," transposition of the senses, etc., etc., he evoked a storm of contumely which upset the balance of his mind, and led to the termination of his existence by his own hand.

At about the same time that Braid began his experiments Dr. Esdaile, in India, in ignorance of those experiments, and under very different conditions, arrived at similar results. After five years' practice he left a record in Calcutta of 261 painless operations of severe character. This direct continuation of the practice, from Mesmer down to Charcot and Liébault, ought to influence our tardy recognition of the source to which honour is due.

A view prevails among the modern differing from that of the ancient professors of hypnotism. The former generally consider the phenomena to be wholly subjective, whereas the latter tended to the belief that they were objective. It is noticeable, however, that the former are not ignorant of the passage of some influence from the operator to the subject, Liébault admitting that "there exists a relation" between them, and Haidenheim when hypnotising looking at the patient "very keenly."

The belief that there is some emanation from the operator resolves itself, in my mind, into a recognition of the telepathic force of will power; that there is a transference of ideas from brain to brain. This transfer of force seems, like the zymotic diseases, to require an incubation period, which incubation period in this instance is characterised by somnolence or lethargy.

The British Medical Association recognises that hypnotism cannot be considered a general remedy of reliable action, and lays down the rule that a stranger should never be hypnotised without a suitable witness. No satisfactory explanation having yet been given, I am justified in assuming that the medical profession has now got hold of something which we do not understand. My mind was a *tabula rasa* as regards this subject, when, fifteen years ago, finding myself possessed of means and leisure, I undertook to study hypnotism and allied phenomena. Attracted by an advertisement, I made acquaintance with an illiterate man named Ashton, proprietor of a sort of hospital for magnetic healing, and took from him a series of lessons in his art. He was able to alleviate pain and disease chiefly of joints, displaying diligent patience and manipulative skill. I was impressed that a force emanated from him. On one occasion when he was declaiming in front of me at some 6 feet distance the rise and fall of his arms (he was a vehement gesticulator) when directed towards me so affected my nerves that I had to ask him to desist. After twelve lessons I found that I could generally relieve slight neuralgias of my friends and family. My opportunities were few. I could not go about gathering patients. The faculty I had acquired became enfeebled and left me through want of use, in accordance with Ashton's doctrine that it could be acquired and cultivated only by constant practice in most instances. After this I went through the whole round of spiritualism that was accessible to me, finding it wholly unprofitable to the mind, until I obtained an introduction to a well-known old gentleman, a Mr. Frederick Hockley, a man of erudition, but apparently of great credulity. His claims to a knowledge and practice of the occult sciences, made to me in confidence, simply, modestly, and as a matter of course, were little less than those of the mediæval magicians. Thirst for knowledge led to my becoming his most intimate friend, to whom he bared his heart, his history, and his lore. I cannot too highly estimate his goodness and nobility of mind. A bibliophite and bookhunter, he possessed an extensive and very valuable library of books and MSS. (now in the British Museum by his bequest), but the series of his own minutely-kept diaries, ranged in handsome bindings on his shelves, inspired me with more interest than all. He had long abandoned all other occult practice to devote himself to what we call "divination" by mirrors and crystals, consideration of which, I believe, will be found to throw a side light upon hypnotism. The notes of his numerous experiences, or, as he called them, "actions," bore by turns upon the most interesting metaphysical discussions, revelations on past, current, and in some instances future events, and scenes and anecdotes like those of an enthralling novel. The key to all the unexplained phenomena revealed by hypnotism and by all his studies is, he taught, to be found in concentration of will power by the operator.

Society has of late years been exercised in mind about thought transference and telepathy. We are familiar with drawing-room experiences of collective will power over one of a company who may thus be impelled to perform almost any incongruous act. I feel convinced that individual will power concentrated upon another by one who is able to use it, and under favourable conditions, must be reckoned remarkable, and in some cases irresistible. A sad case of the unintentional misuse of the influence is within my knowledge. About twenty years ago an officer in garrison at Malta found he thus was able to control the actions of a young member of the ancient nobility of that island, and was accustomed to amuse his brother officers at the expense of that gentleman. He carried it so far as to cause the victim to pick pockets. The officer returned to England, and some time afterward many small articles having been missed from the Club a detective was employed, who traced the thefts to the nobleman in question. The culprit was tried, found guilty, and sentenced, to the disgrace of his family and the ruin of his career.

Obedience by the subject in after days to suggestions given in the hypnotic state appears to be an extension of the influence which we are not able to explain. The supersensuous perceptions alleged to become manifest during the stage of somnambulism appear to be related to those recorded by Frederick Hockley from hypnotism by mirrors and crystals, and are equally unexplained. The evidence for this class of unexplained phenomena is becoming very strong, and rebuts dogmatism on the subject. Bearing in mind the slow stages of official recognition accorded to the simpler facts of hypnotism, we cannot be surprised at delay in the acceptance of its more advanced phenomena. These in former days always ruined the reputation of those who attested them.

A writer in the April number of the *Nineteenth Century* magazine, reviewing the claims of the advanced phenomena and urging them on the consideration of men of science, asserts "That it is possible for an experimenter to produce a hallucinatory image of himself in the perception of a friend at a distance without the friends having received any previous suggestion or anticipation that such an image would appear," and claims it as a fact "attested in several instances by trustworthy persons at each end of the chain." This claim, if it can be substantiated, may be found to correlate the admitted phenomena of hypnotism and telepathy with the astounding claims of the late Frederick Hockley.

Amongst instances where revelations received through his mirrors were confirmed by subsequent independent evidence is that which has been frequently quoted in spiritualistic literature of the minute description of a startling incident which occurred to the late Sir Richard Burton during his journey, in disguise, to Mecca, at a time when the world had given him up for lost. This vision, recorded in Hockley's diary simultaneously with the event, was verified and attested by Burton on his return. In the same diary stand recorded the ruin of the Tuileries and the attendant conflagrations in Paris years before the events of the Commune.

Amongst the array of extraordinary pretensions which the close of this century is reviewing we may detect much fraud, much collusion, imagination, and credulity, yet if anyone will devote three years, as I did, to their patient investigation, I say that he will be unable to deny the presence of some facts whose nature and relations to explained facts he will be unable to explain. And this is the attitude of the scientific world towards hypnotism.

ELECTRIC TRACTION.

By Montague Rhys Jones, C.E.

Whether the present generation has discovered the ultimate force in nature most applicable to the service of man is a question for the scientist of the future to answer. We can, however, claim this to be the electrical age, as in prehistoric times there were ages of stone and bronze, and in introducing the phenomenon of electricity, as applied to traction, we cannot help being forcibly reminded of the unity and continuity of nature, when we consider how the method of applying this unknown force has, like all other physical and social phenomena, "ever from simpler to more complex grown;" it now remains an important part of our environment and if we failed to correspond the result would be a retrograde movement of the human species.

Passing over its agency as a transmitter and reproducer of thought and sound, and other commercial and industrial functions, we come to its latest evolution in its application to the propulsion of railway and tramway cars. Its practical utility was first demonstrated at Berlin, in 1879, by Siemens and Halske, on an experimental line of 500 metres. The train consisted of a small electric locomotive and carriages, which had very small wheels, with two rows of seats running parallel with the rails. The locomotive was fitted up with one of Siemens' dynamo-electric machines, laid horizontally on a frame-work with wheels, the bobbin being parallel to the rails, and the field electric magnets perpendicular thereto. The rotary movement of the bobbin was transformed and transmitted to the driving wheels of the small locomotive by cog wheels, and a bevelled wheel completed the communication of the movement. A bar of iron, to introduce the current, was laid between the two rails, and encased in wood, to insulate it electrically from the soil; "two spring rubbers attached to the locomotive rested on the bar. The current was transmitted by these rubbers into the machine; after having done its work it passed through the wheels of the locomotive, and back to the generator by the iron rails. It was not necessary for the rails to be completely insulated, for if some of the current escaped into the earth, it still returned to the generator, that being equally connected with the ground."

The success of this experiment led to other attempts of an exhibitional nature, at Brussels, Durseldorf, Frankfort, and other places with the same result, and then electrical traction passed from the experimental to the commercial process of development, and the Lichterfelde Electric Tram near Berlin was the first of this kind. The length is one and a half miles, and the equipment in 1881 consisted of two motor cars, the motion being transmitted to the wheels by belts working on grooved pulleys outside the wheels. The prime source of power was a steam engine, with a Siemens' motor and generator; but the installation differed in some respects to the Berlin line, the central rail not being used, but the one rail acting as a lead, and the other as a return for the current. Up to 1887 this line carried a hundred thousand passengers yearly. These instances are merely recorded to show that the inception of Electrical Tramways took place in Europe, the principle being the generation of electricity by dynamo, and conveying the current through conductors connected by sliding contact with the cars while in motion. Modern electrical railways are now built chiefly on this principle, although America has far superseded Europe in the improvement and perfecting of the system.

After the year 1881 the commercial aspect of electric traction was carefully considered, and numerous lines were constructed both in Europe and America. In the former the mines of Zankeroid and Hohenzollern constructed short tramways the system of conductors, being overhead inverted T rails. They each carried 300 tons daily, at a cost of a halfpenny and three farthings respectively. The first electrical tramway in England was projected and constructed by Mr. Mangus Volk, at Brighton, in 1883. It runs along the sea beach for about one mile, and includes some heavy grades and sharp curves. The speed was limited to eight miles per hour, but twenty-five miles has been done. The current is transmitted along the rails, with a gas engine as prime power. One million passengers are carried yearly, at a cost of 1·92 pence per car mile, and nearly 50,000 miles are run during the year. In 1883 followed Port Rush, in Ireland, with a six mile length of single track, the current being sent along a third rail by water power. And here let me state that, in my opinion, both Hobart and Launceston are eminently suitable for electrical enterprise, and that a well digested scheme of electrical tramways could be devised for both places, as would compare favourably in point of economical working with any system in the world, owing to the existence of an abundant and permanent head of water, which could be utilised for the generation of electrical power, but a radical alteration would be necessary in respect to the permanent way which is now being laid down in Hobart. The rolling surface is of such a crude character as will not only seriously affect vehicular traffic, but the successful operation of electrical trams.

In rapid succession followed Blackpool with an underground system, technically known as the conduit system. Mordling, Frankfort-Offenbach, with overhead slotted tubes as conductors. Brussels and Hamburgh followed in 1886 and 1887 with the Juien system of storage batteries, which, as will be subsequently explained, proved a failure. So far as America is concerned she has left us far behind in the race for cheap transit.

The first electric railway in America was exhibted at the Chicago Exhibition, in 1883, by Messrs. Field and Edison, the track running round the gallery of the main building, curving sharp at both ends, with a radius of 566 feet. The total length was 1,500 feet. The gauge was three feet, with a central rail for the current, and the two outer rails for returns. On June 5th, the "Judge" and its car, loaded with sixteen passengers, was started. The railway ran for a fortnight, completing a mileage of 446, and carried 27,000 passengers. The trial was considered a success. Mr. Leo Daft then began experimenting with his electric locomotive, the "Ampere." The actual performance was the hauling of a railway car weighing ten tons with sixty-eight persons in addition to the motor, which weighed two tons and carried five persons. The speed was eight miles per hour, upon a track having a gradient of 93 feet to the mile, and included a 20 degree curve. A maximum duty of about 12 h.p. was registered; and, although the actual efficiency was not determined, the 25 h.p. engine which actuated the primary machine was also doing other work at the factory.

The first electric tram open for traffic in America was the Baltimore Union Passenger Railway, in 1885; it had two miles of single track, six cars, and 260,000 passengers were carried yearly up to 1887, at a cost of 16s. per car per day, each running 73 miles daily; a third rail was introduced as a conductor, also an overhead wire. Electrical progress in America since that date has been simply astounding. In the beginning of 1890, one hundred and fifty towns had electric trams, aggregating 1,670 miles of track, with 2,650 motor cars, equivalent to 70,000 h.p., running 150,000 miles per day, and carrying 200 millions of passengers yearly. The prime source of power required to meet this gigantic traffic is estimated at 40,000 h.p., and it is stated, on the authority of those competent to form an opinion, that 3,000 motors will be at work this year, carrying 3 millions of people, and calculated to return in fares about 3 millions sterling.

It is always a profoundly interesting thing to know something of the historical development of a force which is destined to greatly modify the physical and mental attributes of man, and if an apology be needed for entering so fully into the historical detail connected with the subject, that is my sole excuse.

Before proceeding to describe the methods of application, and so as to afford those who have not had an opportunity of studying the subject, it is proposed to briefly enter into an historical and physical description of the dynamo and motor, two of the most important agents in the application of electricity as a motive power, which will, it is hoped, enable them to have a thorough grasp of the principle.

Before explaining the physical theory of the dynamo, it may not be outside the scope of this paper to briefly refer to Faraday's discovery in 1831 of the laws of magnetic-electric induction. He found that induced currents could be started or stopped in an adjacent coil, which led to the further discovery that currents could be generated in a coil moved in the poles of a powerful steel magnet. He then made what he termed a "new electrical machine," whose parts consisted of a copper disc 12 diameter, and one-fifth of an inch in thickness, fixed on a brass axle, and mounted in frames so as to revolve, its edge being placed between the poles of a large compound permanent magnet. Copper and lead conducting strips were placed in contact with the edge of the disc, a wire from the galvanometer was connected to the collecting strip and the other to the brass axle. On revolving the disc a deflection of the galvanometer was obtained, which was reversed in direction when the direction of the rotation was reversed. Here, therefore, was demonstrated the production of a permanent current of electricity by ordinary magnets. From 1832 many inventions and improvements followed, until 1848, when Brett made the important suggestion of causing the current developed in the armature by the permanent magnetism of the field magnets to be transmitted through a coil of wire surrounding the magnet, so as to increase its action. This improvement marked an era in the evolution of the dynamo, being the first suggestion of the self-exciting dynamo. Then followed a period of great activity, when further improvements were effected, and in 1867 Dr. Werner Siemens described in the Berlin Academy a machine for generating electric currents by the application of mechanical power, the currents being induced in the coils of a rotating armature, by the action of electro-magnets, which were themselves excited by the currents so generated. To mark the importance of this departure, Siemens coined the word "dynamo-electric machine," which now is shortened into "dynamo," and which has now become the name for all these electric machines driven by mechanical power, whether self-excited or not. Brush introduced his famous dynamo of a shunt and series winding for the purpose of enabling the machine to do either a large or small amount of work. Many other Americans produced dynamos, amongst them being Edison. The dynamo was now brought up to a great state of perfection, and since the year 1883 the chief progress made has been in details of design and mechanical construction.

I will now proceed, as briefly as possible, to illustrate the physical theory of dynamo-electric machines, which I hope will enable us to understand more clearly the part they play in the propulsion of tramway cars. Professor Sylvanus Thompson defines a dynamo-electric machine as "a machine for converting energy in the form of mechanical power into energy in the form of electric currents, or *vice versa*, by the operation of setting conductors to rotate in a magnetic field, or by varying a magnetic field in the presence of conductors." Projected on the screen is a magnet with electric tufts from the North to South poles. (Plate I.) If iron filings were placed in these magnetic fields, they would arrange themselves thus:—(Plate II.) Now, if we place a conductor, such as a piece of copper wire, in this magnetic field, as it is technically called, and moved it about intercepting these lines, an amount of electricity would be generated in the conductor at right angles to the course of the movement of the conductor, and also at right angles to the direction of the lines of force. "This induced current is purely conditional on the intensity of the magnetic field, and the length and velocity of the moving conductors, as well as the resistance of the wire to the current." Now, as to what this current is, no one knows. It is one of those ultimate scientific facts at present behind the veil. Here is another diagram showing the magnetic field surrounding the conducting wire, end on. (Plate III.) Every wire, as Professor Thompson puts it, "is surrounded by a sort of magnetic whirl," thus:—(Plate IV.) To do this wants energy, and that constant, if to be maintained. It is these whirls which act on magnets, and cause them to set, as galvanometer needles do, at right angles to the conducting wire. It must, however, be remembered that a moving conductor in its motion must cut the lines of force that pass through the circuit of which the moving conductor forms part. I cannot illustrate this fundamental truth better than by throwing upon the screen a few diagrams of Professor Thompson's, and by quoting him on the subject:—"If a coil or wire circuit be moved along in a uniform magnetic field, as indicated in this diagram (Plate V.), so that only the same lines of force pass through it, no electric current will be generated; or if, again, as in this diagram (Plate VI.), the coil be moved by a motion of translation to another part of the uniform field, as many lines of force will be left behind as are gained in advancing from its first to its second position, and there will be no current generated in the coil; but if, as in this figure (Plate VII.), the coil be tilted in its motion across the uniform field, or rotated round any axis in its own plane, then the number of lines of force that traverse it will be altered and currents will be generated. These currents will flow round the ring coil in the positive sense. 'The positive sense of motion round a circle,' I may here add, 'is here taken as opposite to the sense in which the hands of a clock go round.' If the effect of the movement is to diminish the number of lines of force that cross the coil they will flow round in the opposite sense. If the effect is to increase the number of intercepted lines of force, if the field force be not a uniform one, then the effect of taking the coil by a simple motion of translation from the place where the lines are more dense to a place where they are less dense, as from position 1 to 2 on this diagram (Plate VIII.), will be to generate currents, or if the motion be to a place where the lines of force run in the reverse direction the effect will be the same, but even more powerful."

From the foregoing simple facts some very important consequences are apparent, the principal of which are these:—"Currents can be generated in conductors by setting up magnetic whirls round them. We can set up magnetic whirls in conductors by moving magnets near them or moving them near magnets. This means energy and an expenditure of power. The more rapid the motion the stronger the current, and by using a suitable commutator or guide all the currents, direct or inverse, produced during recession or approach can be turned into the same direction in the wire that goes to supply currents to the external circuit, thereby yielding an almost uniform current."

A very large number of dynamo-electric machines have been constructed on the foregoing principles, and the variety is legion, but the ones most used for the purpose of electric traction are those in which there is a rotation of a coil or coils in a uniform field of force, such rotation being effected (as in this diagram—repeat Diagram VII.) round an axis in the plane of the coil or one parallel to such an axis.

An electric motor is the reverse in its action to the dynamo, the latter converting mechanical energy into electrical force, and the former conversely transforms the current of electricity supplied by the dynamo from electrical force again into mechanical energy, thereby propelling the cars. The electric motor "includes all apparatus through whose employment work is performed." Everyone, no doubt, has heard of "Barlow's wheel." This was the first elementary electric motor. He found that by passing a current from the centre to the circumference of a copper disc between the poles of a magnet the disc would revolve. Faraday, you will have observed, in his researches in induction, reversed Barlow's experiment, but those discoveries resulted in the dynamo. It was not until 1873 that the reversibility of the dynamo was discovered, although for many years previous to this motors for driving machinery were operated, but depended on the galvanic battery for a supply of current. This process, being a very limited one, went to the wall when a cheaper current was effected by the discovery of the dynamo. The action of the practical and commercial motor now used is that of a magnet and a wire conveying a current, and is the reverse action of the dynamo, in which the motion of the conductor generates the current. On the other hand, a current in the conductor creates motion.

The law of the survival of the fittest has been an important factor in reference to types and varieties of the motor, not only in many mechanical appliances, but other resources resorted to to intensify the currents.

Having, it is hoped, given a clear insight into the theory of these electrical machines it is to be regretted that the limits of this paper will not permit a detailed description of the mechanical parts (Diagram of Sprague's Motor—A); but so long as the elementary truths have been retained, and that they will be brought to bear on the more descriptive portions of this paper, the object of the same will have been attained.

There are two methods of applying electricity for traction purposes—(1) by storage batteries; and (2) by metallic conductors over and under the track, known as the "overhead system" and the "underground" system respectively. These methods, however, are both of a direct nature, the motors being supplied with current from the conductors as wanted. As an ideal method of propulsion the storage or accumulator system stands pre-eminently foremost, and the best electrical intellects of the day are devising and improving with a view to eliminate the many defects which prevent it from being introduced commercially. The batteries occupy but little room, and are placed under the seats, and the motor is placed under the body of the car. There is no smoke or dust or accompanying noise and no street gearing, and the æsthetic sense is greatly conciliated by its attractive and graceful movement without contact. This is all very well on paper, but when brought into actual experience, like all ideals, it falls short of expectation. The serious objections to its commercial employment are that the accumulators are composed of lead, weighing altogether about 3,500 lbs. in an ordinary street car, thereby reducing the electrical efficiency by nearly 60 per cent. These cells, stored with power, have to be always carried about, whether over heavy or light grades, and if the car has to surmount any grade over 5 per cent., which is common in streets, the capacity and discharge rate is so limited that it is with only great difficulty and serious injury to the mechanical parts that they can be negotiated, and the chemical energy of the cells instead of supplying current developes heat and buckles the cell plates. Platinum or gold might withstand this constant molecular activity; but then, again, it does not do away with the dead weight, to say nothing of expense; but I believe gelatinous cells are spoken of as being highly probable in place of the lead cells. Last January I was asked to value the assets of the Eaglehawk Electrical Tramway Company, which was operated on the storage system for about three months or more, and which proved an entire failure. Its cost amounted to £40,000, and the debris was worth about £5,000. It could have been nothing but ignorance that suggested the storage system for Sandhurst, as the street conditions were entirely unfavourable for such. It is indeed difficult to speculate what the ultimate destiny of the storage car will be, but if improved up to present commercial requirements the most perfect ideal of street transit will have been accomplished.

The underground system, in point of construction, is very much like the cable system, so far as relates to the conduit for carrying the conductor, and the chief objections to it are cost of construction. The conduit in a busy thoroughfare would probably have to be excavated without the use of explosives, at a great cost; and without a large wetted perimeter is allowed for during heavy rains the channel would be flooded, and the water and street debris would come in contact with the conductor, which cannot be fully insulated owing to contact with the trailer, which is suspended from the car, and from which the motor takes its supply of current. The efficiency is entirely destroyed by short circuits and leakage. There are many other mechanical difficulties to be overcome, and one of the great drawbacks which militate against its success is the large amount of ironwork on the surface of the street, together with difficulties of switching and cost of maintenance.

We now come to that system which has been most successfully operated, commercially and practically, throughout the world, and whose adherents and supporters are growing yearly, namely, the "overhead system." Overhead conductors consist of two elements, one having a metallic circuit of two parallel wires, and the other only one overhead wire, the rails and earth forming the return current. There are various methods of contact between the car and the overhead wire, but the one universally used is the single wire under contact system, with bearing wires suspended crossways in narrow streets and centre pole suspension in wide streets, the chief points in favour of these arrangements being the neatness and simplicity of construction, the ease and perfection of all switching, and the well designed and permanent attachment of the trolly arm, a balanced and pivoted pole attached to the car rendering the danger of a falling trolly a very remote contingency. (Plate VIII.)

There are two prominent firms in America who by their great ability and energy have been instrumental in carrying out most of the electric roads in America. I refer to Sprague and Thomson-Houston. There is little in point of difference between the systems of either firm, but I shall describe Sprague's system as a type of this modern electrical railway, having been in communication with that firm in reference to the design of a plant for an electrical tramway, whose conditions were of an exceptionally difficult character, there being a grade of 1 in 8½ for a distance of eight chains.

We have to consider five points in connection with the overhead system:—1. The power-house or generating station. 2. The conductor from the power-house to the car. 3. The motor attached to the car. 4. The connection between the motor and the axle of the car. 5. The return of the current to the power-house.

Thrown on the screen is one of Sprague's power-houses (Plate IX.), supplied with Armington and Sims' engines connected up to the dynamos, and thereby supplying the necessary mechanical power for the production of the current, and in designing these engines it is desirable that they should be so constructed that no variation of more than 2 per cent. in the speed should take place, whether there is a small amount of work to do or whether they are called upon to develop their highest capacity at any given moment, and it is always necessary to have the chief parts in duplicate in case of accident.

The conductor from the power-house to the car has undergone many modifications, but the plan which is now generally adopted is a trolly wheel, firmly fixed to a balanced and pivoted pole having a universal movement. This projects from the roof of the car, and is kept in contact with the conductor or overhead wire by aid of springs from below pressing it underneath and against the overhead wire.

With regard to the overhead work a three-tenths of an inch copper wire is stretched over the centre of the track. At every 120 feet it is suspended about 20 feet above the level of the street. This is done either by means of a bracket, as shown on the diagram (Plate X.), or from a thin cable stretched across the street from kerb to kerb, attached to poles either of wood or iron. This, of course, necessitates the insulation of the main wire by means of a small insulator made of mica and indiarubber.

The poles are planted in concrete to a depth of 6 or 8 feet and a good earth connection made, so that leakage of current can be grounded. The poles are very ornamental in design, and are capable not only of any amount of artistic embellishment, but can be practically used for electric lighting. On short roads the overhead wire provides current for the whole of the motors on the track, but on long lines, where traffic is heavy, feeders are resorted to, carried overhead or underground, and connected with the main overhead wire at intervals of 400 to 500 feet by small lateral or sub-feeders. The important features of this excellent system are the maintenance of a constant potential along the entire length of the road, avoiding a breakdown of the system in case of fires, congested street traffic, or the rupture of the overhead lines. Traffic would still be carried on at either side to the breach, or if by placing cross-over roads in the track at the ends of these sections the inconvenienee would be slight.

The next point to be explained is the motor and the connection between the motor and the axle of the car. (Plate XI.) You already know the main functions of the motor. The current is conducted to the motor under the floor of the car. The mechanism consists of a motor fixed under frame of car. On the end of the motor shaft a pinion gears into a spur wheel on an intermediate shaft. On the other end of the latter another pinion gears into a spur wheel on the driving axle of the car, and motion thereby being communicated. The Sprague motors are perfectly automatic, running at nearly the same speed for all roads up to the maximum, and adjusts itself to normal speed under sudden changes in load. A diagram showing a street railway current curve gives an idea of the erratic power-calls on the motor. (Plate XII.) As regards the system of braking, when a motor is in operation it is generating an electro-motive force. In other words, it is acting like a dynamo, and since this depends upon the strength of the field magnet and the intensity of motion, and since the field magnet strength is under positive control, it follows that the electro-motive force can be made to equal the initial motive force, and even to exceed it when this electro-motive force of the motor thus predominates. The machine will then become a generator and give current to the rails, and its mechanical effects are reversed, so that it brakes the train instead of propelling it.

For controlling the cars a series of levers are worked on the platforms, and a "rheostat" is used for throwing machines into circuit. The last item to be determined is the return of the current to the power-house. There are many methods, as have already been pointed out, namely, by earth plates being buried in damp soil and by rail; but in recent practice the complete metallic return, the same as the overhead wire laid on the top of the sleepers and between the rails, and connected with the latter close to the joints, is pronounced to be satisfactory.

Of course many objections of an æsthetical nature have been urged against the overhead gearing, but they are more apparent than real, and those who take their stand on such paltry objections I would recommend to look at the telegraph and telephone cables so obtrusively conspicuous in big towns. The telephone people have also complained that the ground return interferes with the successful operation of the telephone. The same argument could be brought against electric lighting and telegraphs. The telephone people could meet this chief objection by having a metallic return constructed, the cost of which would be a mere bagatelle. As regards the risk to human life by shock, the working potentiality of 500 volts is so low that it is hardly worth considering. Having touched the fringe of the principal objections a brief comparison, *pro* and *con*, with other systems of traction will be considered. Traction expenses on horse tramways amount to two-thirds of entire working expenses, to say nothing of its inhumanity; and as to steam, it is a clumsy but effective method of applying force, and the citizens of Hobart would be wanting in public spirit if they ever permitted steam engines to run along their streets. If a draughtsman sat down and deliberately attempted to design an affront to a decent community he could not have succeeded better than at Sydney. The working expenses of the steam trams there amount to 3s. per train mile. It is now generally admitted that the struggle for existence lies between the cable system and the electric. The cost of the construction of the former in Melbourne amounted to £34,000 per mile, and 75 per cent. of the available energy is lost by dragging the cable itself, while the whole of the energy can be utilised by the overhead system, excepting a few points per cent. due to resistance of the current by the overhead wires, to say nothing of numerous mechanical defects. At Minneapolis £80,000 was spent in the purchase of cable plant, but was cast on one side as scrap iron to make way for an electrical system. The cable system can certainly surmount phenomenal grades, but on the other hand the electrical tramway can negotiate 1 in 8 grades, or even less; it is only a question of power; and Mr. Reis, an electrical engineer, has made some very valuable discoveries as to electrical braking and adhesion, which I hope to see practically demonstrated at an early date.

As much as one would like to submit these different tramway systems to a searching analysis of cost, not only as regards construction, but working and maintenance; but having already overstepped the limits of a paper, I can simply record the fact that the Frankfort-Offenbach line in Germany is the most expensively worked tramway in Europe or America, the cost amounting to 4½d. per car mile. Mr. Crosby summarises the cost of working, etc., three of the principal lines in America :—"Interest on investment, one-quarter to one-fifth of the whole, *i.e.*, 1 cent per car mile, or, say, 20 per cent. of the total. Coal is about 12 per cent., attendance 40 per cent., machinery and line (without interest) about 20 per cent."

Here is a view of the Telpherage system. (Plate XIII.) It requires no earthworks, bridges, culverts, etc., as railways do. There are three lines worked on this system in England—Alexandra Park, half a mile; Glynde, a mile and a half; and Eastpool a mile and a half; the latter two for mineral purposes. I throw the suggestion out that they are admirably adapted for the developments that are taking place at the present time on the West Coast. It is difficult indeed to surmise what shape electrical developments will take in the future, its potentiality being apparently infinite. It is, however, no stretch of the imagination to say that it is the locomotive power of the future. In preparing this paper I have consulted Professor S. P. Thompson's works, Professor Ayrton, Du Moncel, Martin and Wetzler, and other scientific papers. (Plate XIV.)

DISCUSSION.

Mr. Macfarlane said :—Your Excellency and Gentlemen, —Having been asked to take part in the discussion following Mr. Montague Jones' excellent paper, I have pleasure in doing so, as I have had opportunity of gathering some information on this interesting subject, being in correspondence with a manufacturing company, makers of electric railway plant, the Thomson-Houston Company. There are only two or three points which I would desire to emphasise as of special general interest to the public, and the first point is that electric traction is not now confined to the region of scientific experiment, but is an established commercial success. In the United States there are 310 tramways or railroads worked by electricity, with 4,000 cars and 7,000 motors. In the city of Minneapolis a new and expensive cable has been abandoned and electricity adopted, and it is estimated that one-third of the street railway mileage in the States is worked by electric traction; and further, the following tramways, amongst others, pay a dividend of 8 per cent.:—The West End Road, Boston (probably the largest tramway company in the world), the Springfield, Troy and Lansenbury, and Utica and Mohawk tramways. These facts are sufficient to show that electric traction for tramways and railroads is a commercial success. I am speaking now of the overhead system, and on the general principle, for, of course, there are electric railroad companies which do not pay dividends, as in all commercial ventures. With regard to steep grades I may add 1 in 7 is not too steep for working. I believe the steepest grade in the proposed Hobart line is 1 in 16.

It is interesting to note further that electric traction has also been successfully applied to tramways for mills and manufactories, as well as to street railways, and is found a convenient method of transporting raw materials, manufactured products, coal, and other commodities from one part of the works to another. Electricity fully meets the requirements of such a case. The special advantages claimed for electricity over steam motors or locomotives are three in number, viz., safety, the removal of obstacles presented by grades, and economy. Its safety is seen from the fact that the road may be run through the mills and store-houses without increasing the insurance rates. Steam is unsuited to indoor work, and even when used entirely out of doors there is always some danger of fire, and there is the smoke and the noise and dirt arising from the locomotive. In the matter of grades, whilst 4 per cent. (1 in 25) is about the maximum which steam can overcome, grades of 12 per cent. (1 in 8½), or more, are easily surmounted by electricity. The expense for horse-power of running a steam locomotive in comparison with a large stationary engine, where all the advantages of concentration are available, shows decided economy in favour of the large plant. Electricity furnishes a means for transmission of power with minimum loss and cost, permitting the concentration of the generating plant, whether steam or water power, and thus appeals strongly to the business man on the ground of economy. One advantage possessed by electricity alone is that the overhead wire may be tapped anywhere, and a stationary motor, an electric hoist, a pump, or any form of electric-power machine operated from it. Electric traction is in use at the works of the Baltimore

Sugar Refinery, Pittsburgh Plate Glass Company's, Trenton Iron Works, and many other mills and factories.

As to the safety of the overhead wire system in regard to freedom from accident or danger to human life, it is very satisfactory to have reliable and independent testimony apart from experts or officials connected with manufacturing companies. The *Boston Daily Advertiser*, the well-known Conservative journal of a Conservative city, addressed letters to the Mayors of the various cities in which the overhead system of electric tramways was in use. The questions submitted were:—1st. What system of electric cars is in use in your city? 2nd. Whether any persons have been seriously injured or killed? 3rd. Whether any apprehension is felt among your people as to safety on account of this electric system? 4th. Whether, as a result of its introduction, the street car service is improved? 5th. Whether popular feeling is in favour of the overhead wires, or hostile to it? In reply 69 answers were received from as many cities, which were published in the *Advertiser*, appearing on the 26th August, 1890. In regard to the third, fourth, and fifth questions the consensus of public opinion is exceedingly favourable; and on the point of danger raised by the second question the *Advertiser*, in summing up, says:—"We find that of the 69 cities reports of accidents are confined to 15. Of these only 8 resulted in serious or fatal injuries to human beings, the others being the killing of horses by falling wires. Of the human beings killed, in all but two cases the accident was due to other causes than the electric wires, and in both cases where the wires apparently brought death they were by electric light wires. We have not heard of a single death from the trolly wire." It must be admitted this independent testimony is very satisfactory.

Nashville, Tennessee, claims to have the largest electric railway system in the States, excepting the city of Boston. And I will conclude this short sketch with an extract from the Mayor of Nashville's letter in reply to the questions submitted by the *Boston Daily Advertiser*:—"No person has been injured by the system. Three horses have, however, been killed, caused by rusted telephone wires falling across the trolly wire and conveying the current from the trolly wire to the ground. In one instance when one of these accidents occurred a Negro woman, in attempting to pass, grabbed"—[Excuse the expression; it is not mine, but the worthy Mayor's.]—"the telephone wire, and was thrown to the ground. She naturally did right smart 'high kicking,' but she immediately arose, much frightened, but in nowise injured. From this it seems that while the trolly wire conveys sufficient electricity to kill a horse, yet there is not sufficient current to kill a human being."

Mr. C. W. S. James, C.E., said he had been deeply interested in the paper read, and had tried to think how far the electric traction system could be applied to Hobart. A few months ago he had estimated what could be done in Launceston by utilising the South Esk River, and he thought that there from 1,000 to 1,300 horse-power could be available. In regard to Hobart, however, there was not the same power available, and he believed they would require to look to generated power in any electrical system introduced. He thought the system could be usefully applied to carrying minerals on the West Coast, and that the "overhead system" was well adapted for the streets of Hobart.

Mr. J. Fincham, C.E., said he had followed the paper with interest, but they had no reference to the length of the steep grades surmounted. He believed that in any scheme for electrical traction in Hobart this would be one of the difficulties. He was aware that there were about 300 tramways in America, but he had not been able to get any particulars as to the grade, or whether it was equal to such a grade as they had to contend with in going up Elizabeth-street.

Mr. A. W. Lawder, C.E., thought the length of grade to be overcome simply resolved itself into a question of the power of the accumulator.

Mr. Fincham said that was just the difficulty the engineers found. They had not been able to provide enough accumulation to overcome a sustained strain.

Mr. Jones, in reply, said Mr. Fincham appeared to associate the "storage system" with the practical operation of electric trams, but it was clearly pointed out that the "storage" up to the present time was an ideal method, and that the "overhead system" was the only one of practical value. The main defects of the former are the limited capacity of the cells, and when brought to bear on a grade of 5 per cent. heat instead of current was developed, which buckled the cell plates, rendering them useless; and, again, the cells are composed of lead, weighing altogether 3,500lb., which has to be carried about over light as well as the heavy grades, losing 60 per cent. of the available efficiency. Coming to the "overhead system," the question of surmounting heavy and continuous grades resolves itself into a question of increased power, which is always to be obtained from the central station, and herein lies the great advantage the overhead system has over any other. All the available energy can be utilised excepting a few points per cent., due to the resistance of the current by the conductor. The maximum grade in Hobart is 1 in 16, and is slight compared with some of the towns where cars are being operated by electricity. The following is an extract from a letter from the directors of the "Richmond Union Passenger Line" to Mr. Sprague:—"The road which you have equipped under most trying conditions has been one of the most, if not *the* most, difficult which could be met with in street railway work. The excessive and continuous grades, the numerous sharp curves, the gradients in these curves, the weight of the cars, and the heavy loads which they have been required to carry, together with the extent of the system, and the number of the cars in operation (about 40), constitute the enterprise the largest and most difficult yet inaugurated in any part of the world. We acknowledge the successful fulfilment of all the terms and conditions of the contract, and compliment you upon having achieved so signal a success." The number of street electric railways at work, and the number contemplated, is quite sufficient to prove that it has long since passed the experimental stage of development. Citizens of Hobart should keep these facts in view, and refuse to listen to any argument stating it to be impracticable to introduce an electric service here. The wretched apology for a permanent way will certainly have to be removed, and a neat steel-grooved rail put in its place, as the basis of efficient street transit is a smooth and sound rolling surface. I would also like to see a 4ft. 8½in. gauge, instead of a 3ft. 6in. gauge, as the latter might create mechanical difficulties which cannot at present be foreseen, as all the electric roads, or nearly so, are built to 4ft. 8½in. requirements. Of course these difficulties would be overcome; still it is just as well to be on the safe side. As regards a prime source of power for Hobart, it is a simple question of cost and maintenance whether water or steam would be better. There is plenty of water, and the cost to supply a constant head would be, I think, much less than coal Two hundred h.p. would be more than sufficient force for a service of 12 cars, and as to the mechanical power for equipment it does not make much difference whether the grades be 1 in 5 or 1 in 10, as the motors used are of a standard size, having a capacity of 15 h.p. each. Where the grades do not exceed 4 to 5 per cent., one motor could do the work, but where the grades exceed 5 and run up as high as 10 per cent., then it is unsafe to operate, except by driving both axles, and then the equipment must be two 15 h.p. motors. The average weight for street motors' equipment is about 100lb. per h.p Hence 2·15 h.p. motor equipment will weigh about 3,000lb. I have deduced this to mathematical formulæ following:—H.P.=4·75=M.T. (C x 1), where M=miles per hour, T No. of tons, C rise in feet per 100. The cost of single line "overhead" construction, including permanent way material, under average conditions should not amount to more than £3,500 per mile. This, in comparison with the cable system at Melbourne at £34,000 per mile is very marked, and the economy of the overhead system is so manifest that Mr. Henry Peabody, of Boston, wrote to me as follows, in reply to inquiries:—"There is a feeling among all Municipal Councils, where railways apply for overhead lines, that the increased economy warrants their asking for a decrease of fares, hence their desire to keep quiet about their balance-sheets." Mr. James asked me to explain the Telpherage system. There are two, "the series," and the "cross ones parallel." The latter is now being operated in many places. The skips or trucks are suspended and supported by iron rods, which are likewise supported by poles, and the lower rod, acting as a conductor, is constructed on the "break and make" principle at every 120ft. or so, taking its supply of current from a dynamo fixed at a convenient place. The "makes and breaks" are normally closed, so that a current of electricity may flow from end to end; but when the first wheel of the skip of a train touches the "break," the circuit is closed, and the current runs back to the last wheel of the train, and into the skip containing the motor, and thereby energising the train. The same operation continues at the intervals stated to the end of the journey. It can be worked up to 15 miles per hour with ease, and the cost of carrying is about one halfpenny per ton per mile. Unlike most new inventions, "Telpherage" does not persist in adhering to any principle of an obsolete type, but is an innovation so extraordinary as to pass all practical experience. As feeders to the West Coast lines nothing could be more admirable, and not in the remote future we shall see it universally adopted, instead of those useless unpayable lines and costly roads that cripple the resources of new countries.

NOTES ON THE SHELL-MOUNDS AT SEAFORD, LITTLE SWANPORT:

By Alfred J. Taylor, F.L.S., F.R.G.S.,E.

Among the many interesting relics of the Aborigines of Tasmania that yet remain, not the least interesting are the shell-mounds that mark the spot where they formed their encampments and feasted before the intrusion of a white race had disturbed their simple and peaceful modes of living. Such shell heaps as that to which I am about to refer occur in other parts of the world, and for some time they were supposed to be nothing more particular than ordinary "raised beaches." Even now in Tasmania there are many well-informed people who cannot bring themselves to believe that the shell heaps noticeable in many places on our shores are the kitchen-middens, or "refuse heaps," of the Aborigines of the colony; and it is for the information of such sceptics that I place on record the following evidence as establishing beyond all reasonable doubt the fact that they are this, and nothing more. One has only to examine these remarkable accumulations to obtain evidence of an intensely interesting and convincing character, and the results of a personal examination of the extensive shell mounds to be found on the estate leased by Mr. Samuel Drake at Little Swanport have induced me to hope that a few notes on the subject may be of some interest to the Fellows of this Society. It may be as well, I think, to prelude what I have to advance as direct evidence in favour of my contention by briefly referring to one or two of the reasons why the accumulation of shells at Little Swanport cannot be regarded as having been due to the upraising of oyster beds, or as having been due to the wash of surf upon the spot. In the first place, although it can be shown that at some places where shell mounds occur in the colony live and dead shells are thrown up by the surf, at Little Swanport there is no wash or surf to cast up shells, for while the plunging seas roar on the bar outside, the waters within are, as a rule, as still and as peaceful as the waters of a mill pond. The conclusion that the shell beds have been upraised from the sea is disposed of by the natural features that distinguished true raised beaches from kitchen-middens or refuse heaps. Sir John Lubbock, in his "Prehistoric Man," has drawn attention to the fact that raised beaches contain species the individuals of which are of different ages, and that the shells are, as a matter of course, mixed up with a large amount of sand and gravel. He also quotes Professor Steenstrup, who, in combating the idea that the shell mounds of Denmark were raised beaches, directed attention to the circumstance that "the shells belonged entirely to full-grown or to nearly full-grown individuals, and that they consisted of four species which do not live together, nor require the same conditions, and would not, therefore, be found together in a natural deposit; and thirdly, that the stratum contained scarcely any gravel, but consisted almost entirely of shells" (pp. 224-5). This reasoning applies exactly to the shell mounds at Little Swanport: for not only is there an absence of anything like stratification or admixture of much sand or gravel with them, but the whole of the oyster shells that have come under my notice are of mature growth, and are mixed up with other species that do not live together. In connection with this negative evidence I may throw out the consideration that if these deposits were merely the indications of an upraising it would only be reasonable to anticipate that many of the shells would be found in natural pairs. Such, however, is not the case at Little Swanport, the shells that I noticed during my recent visit to that very pleasant and interesting spot being, in every instance, single shells scattered just as we might expect to find them in refuse heaps. Another noticeable feature in the mounds at Little Swanport is the fact that a very large proportion of the shells are broken at one particular part, as if they had been broken there to assist in the process of opening them. It is also noticeable that many of them have undergone the action of fire. To turn to the more direct evidence. The encampments of the Aborigines were always formed on the margin of streams or lagoons. Possessing no appliances for digging or sinking wells, it was a matter of importance to them to be near fresh streams or springs. On the spot where the shell mounds are thickest, at Little Swanport, there is a splendid fresh water spring, which bubbles up within a few yards of the bay, yielding a plentiful supply of the sweetest fresh water. Their place of encampment depended also upon the food obtainable, and as the daily food of those living near the Coast consisted largely of shell fish, such as oysters, turbos, and haliotis or mutton fish, they would naturally resort to such a favoured spot as Little Swanport, where these, together with the crayfish, another article of their diet, could be found in abundance. As a matter of history, it is known that a very large tribe of natives, known as the Oyster Bay tribe, frequented the spot, and during my recent visit to it I was able to discover some very interesting indications of the primitive conditions under which they existed. When I first decided to turn the shell heaps into lime of commercial value, it struck me that many articles of ethnological interest might be discovered, and I gave strict instructions to the parties employed to send me all bones or remarkable stones that they might come across in breaking down the shells for the kiln. It was not long before I had the pleasure of receiving a parcel of bones—the remains of the marsupials the natives used to feed upon—and I at once noticed that the larger ones had been split, evidently for the purpose of extracting the marrow. At a meeting of the Royal Society of Tasmania in 1873 Mr. James Scott volunteered certain information received from his brother, Mr. Thomas Scott, who, among other things, referred to a stone which he had sent to England—"A round stone chipped all round to a circle about 7in. in diameter, and 1½in. thick in the centre, to 1in. at the edge. On this the females broke the bones of animals for the marrow, using another stone about 6in. in diameter for striking." I am able to exhibit a stone exactly answering this description, which I took from the surface of one of the shell-mounds. It will be noticed that at the middle part on one side it has been worn smooth by the friction of use. I also found mixed up with the shells other round water worn stones, evidently used for the same purpose; for in the Transactions of the Ethnological Society for 1863 there is a statement made by Mr. Milligan to the effect that the Aborigines had assured him that round stones of different sizes, together with chipped flint instruments, would be found in the shell-mounds left on the scenes of their feasts. Bearing in mind the latter portion of this statement, I was very anxious to find some of the chipped flints, and made a careful examination of the surface of the shell-mounds. You may imagine my delight when my eye rested on a well marked one which had been turned up in ploughing. Further examination led to the discovery of several others of the same character. But I was not yet satisfied, as I had found them all upon the surface. Being anxious to test the extent of the shell deposits, I got my companion, Mr. W. Luttrell, to put down a few prospecting holes, and in sinking one of these we came across a well-chipped flint, buried in the shells some 12in. below the surface. There is one significant fact in connection with the bones found in these shell-mounds, and, indeed, with the shell heaps alone, and that is the constant occurrence of charcoal—the remains of the fires kindled by the natives to roast the animals and to cook the shell fish "in their own gravy." It is known that to do the latter the large shells were placed upon the fire as dishes, and that this method of cooking rendered the contents tender and succulent. The presence of the charcoal under such circumstances affords another link in the chain of evidence that bears out the certainty that these shell-mounds are something much more interesting than upraised beaches or oyster beds. Perhaps the most interesting of the "finds" yet made in connection with the shell-mounds at Little Swanport has been the discovery of a bone (which I now exhibit) which gives evidence of having been worked or ground. As far as I know it is the first bone instrument of the kind that has yet been referred to in connection with the history of the Aborigines of this colony. It was discovered by Mr. Herbert Luttrell in breaking down the shells from one of the faces opened to supply the kiln. Mr. Ronald Gunn has stated that in removing haliotis or mutton fish from the rocks to which they closely adhere the natives used a wooken spatula-shaped instrument, and Backhouse writes that the mutton fish were often taken in deep water by the native women, who dived for them and forced them off the rocks by means of a wooden chisel. The French explorer, La Billardiére, saw how these chisels were made. "We observed," he writes, "some of the savages employed cutting little bits of wood in the form of a spatula, and smoothing them with a shell, for the purpose of separating from the rock limpets or sea-ears on which to feast." I think it quite probable that the bone instrument before us was used for a similar purpose. On examination it will be found that it is somewhat worn just at the spot where it would be most likely to show the effects of friction if used to work and lever the sea-ear from the rock. In any case it is interesting, as indicating one step towards a degree of civilisation that the Aborigines of the colony were destined never to reach. In putting down the prospecting holes above referred to I satisfied myself that the shell deposits at Seaford extend over several acres of ground, varying in depth from 6in. to 6ft. It is the vast extent of these deposits that renders it almost impossible for anyone at first blush to realise that they are the refuse heaps of the Aborigines. When it is remembered, however, that the tribes near the sea-shore lived principally on shell fish—for, according to Calder, and other authorities, the natives of Tasmania would rather have starved than use any kind of scale-fish for food—it will easily be seen that large refuse heaps would very rapidly accumulate on the feeding grounds. The best authorities agree that the natives were very ravenous eaters, and setting the number of the Oyster Bay tribe at 500 (an estimate I have heard given), and putting down to the account of each member of the tribe a consumption of 20 oysters per day, that small calculation alone would give 20,000 single shells per day to the refuse heaps. There is one thought in connection with this matter with which I will conclude my somewhat desultory notes. Lubbock has pointed out that—"A country, the inhabitants of which live by hunting and fishing, can never be thickly populated." Regarding the shell-mounds at Little Swanport in the light of this fact, and taking into consideration their vast extent, the conclusion is forced upon us that the spot must have been frequented by the natives for a very long period of time, and this is a fact that should not be lost sight of when efforts are being made to trace their history and origin. It now only remains for me to mention that we are about to turn these interesting shell-mounds into a valuable article of commerce, and should anything of interest be discovered during their removal, it will give me great pleasure to make the Fellows of this Society acquainted with the fact.

DISCUSSION.

Mr. R. M. Johnston said he was very pleased to find Mr. Taylor had taken so much trouble in obtaining such valuable information dealing with the Aborigines of Tasmania. The paper read by Mr. Taylor was extremely interesting from an ethnological point of view. He, Mr. Johnston, had made a close study of these native "kitchen middens" for many years, and in his work on "The Geology of Tasmania" he had devoted a considerable space to the description of these interesting deposits so common along the borders of our estuaries and coasts. The particular shell deposit at Little Devonport, referred to by Mr. Taylor, had been investigated by him about two years ago, and his observations then made, in company with Mr. Morton and other members of the Fishery Board, left no doubt in his mind that the deposit was simply one of the many kitchen middens of the Tasmanian Aborigines abounding everywhere along our coast lines in similar situations. The lenticular layers of ashes and charred wood, intercalated with the shells, were then observed by him, and pointed out to those who were with him as proof of their accumulation by human agency. He could therefore confirm the accuracy of Mr. Taylor's descriptions and conclusions. Anyone who has had the slightest experience in observing these interesting deposits, now fast disappearing or being obliterated by the agriculturist's plough and other agencies, would never confound them with the raised beaches so common in Bass Strait Islands and along portions of the northern coast.

Mr. Johnston then drew attention to the numerous kitchen middens existing all along the borders of the estuary of the Derwent, and especially along the Domain, New Town Bay, mouth of the Jordan, One Tree Point, and Blackman's Bay. These are generally formed on little knolls or headlands, commanding a good outlook on every side. One interesting mound exists high up on the Domain, near the New Cricket Ground. From all of these he had collected the chipped flints, and occasionally split bones of the wallaby. Over 150 chipped flints had been collected by him from these deposits near Hobart, and a very large number had recently been sent to him, collected by Mr. John Morrisby on the borders of Pipeclay Lagoon, South Arm.

He stated also that he had recently received a communication from Mr. Mitchell, of Lisdillon, who tried upon the basis of the extent of these shell deposits to make some estimate as to the length of time during which the Island was occupied by the last Tasmanian race. Mr. Johnston stated that an estimate formed in this way would be very unsatisfactory, as the kitchen middens are formed for the most part on the marginal boundaries of our seas and estuaries, and are constantly being encroached upon by the great invader, the sea. It is evident therefore that the horizontal encroachment of the sea would have destroyed the very earliest deposits, supposing them to have existed. It is almost certain from ethnological considerations that the Tasmanian Race must have existed in Tasmania for a very long period, for otherwise how can we account for the marked racial type as compared with the natives of the Australian mainland and elsewhere.

AMERICAN MUSEUM OF NATURAL HISTORY NEW YORK

REPORT

OF THE

ROYAL SOCIETY

OF

TASMANIA

FOR THE YEAR

1891.

Tasmania:
WILLIAM THOMAS STRUTT, GOVERNMENT PRINTER, HOBART.

1892.

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MINUTES of the Annual General Meeting of the ROYAL SOCIETY OF TASMANIA, *held at the Royal Society's Rooms on Tuesday evening, 5th April,* 1892,—JAMES BARNARD, *Esq., Vice-President, in the Chair.*

THE following gentlemen were elected Corresponding Members:—Messrs. A. Sydney Oliffe, Government Entomologist, N.S.W.; G. M. Thomson, F.L.S., Principal of the High School, Dunedin, N.Z.; J. Shirley, B.Sc., of Brisbane, Queensland; W. M. Hamlet, F.C.S., F.I.C., Government Analyst, Sydney; Professor W. Bragg, M.A., of the Adelaide University; T. W. E. David, F.G.S., of the Sydney University; W. Baldwin Spencer, M.A., of the Melbourne University; and Dr. A. Dendy, D.S.C., Melbourne University.

The following persons were elected Fellows of the Society:—John Deans, M.D., Miss May Dickson, the Rev. Herbert R. Finnis, and Messrs. Henry H. Anderson, B.A., and John Goodall, C.E.

REPORT.

The Council of the Royal Society of Tasmania have much pleasure in submitting the following Report for 1891.

The first monthly meeting took place on April 22nd, when the President, His Excellency Sir R. G. C. Hamilton, K.C.B., occupied the Chair, there being a good attendance of Fellows, Lady Hamilton and several ladies being also present. Mr. A. Morton, who, in conjunction with Mr. C. H. Grant (then absent from the Colony), had been appointed as Delegates from the Royal Society to the third meeting of the Australasian Association for the Advancement of Science held at Christchurch, New Zealand, in 1891, gave an interesting account of their proceedings, reporting that Hobart had been fixed as the next place of meeting, and that His Excellency Sir R. G. C. Hamilton had consented to fill the important office of President.

Six meetings of the Society have been held, that of November having been postponed owing to the near approach of the Science Congress in January. All the meetings had been well attended by the Fellows.

Thirteen papers have been read, on many of which interesting discussions took place. His Excellency had been present in the Chair on all occasions. The subjects of the papers read were:—

By H. H. Montgomery, D.D., Bishop of Tasmania.

1. "Account of the Mutton Birds or Sooty Petrels, as seen in their homes among the Furneaux Islands, Bass Straits, Tasmania."

By R. M. Johnston, F.L.S.

2. "Notes on a Collection of Plant Impressions from the Henty River."

3. "Observations on the Causes of Elevation and Subsidence of the Earth's Crust."

By Baron von Mueller, K.C.M.G.

4. "Remarks on some Rare Tasmanian Plants."

By A. B. Biggs.

5. "The Possibilities of the Telescope."
6. "Total Eclipse of the Moon, May 24, 1891."
7. "The Transit of Mercury, May 10, 1891.'

By F. Danvers-Power, F.G.S.

8. "Notes on the Mount Lyell District, No. 1."
9. "Notes on the Mount Lyell District, No. 2."

By F. J. Jacobs, A.T.A.

10. "Note on the Arithmometer as an aid to Actuarial Work."

By W. Spencer, M.D., M.R.C.S. Eng.

11. "Hypnotism and alleged Phenomena."

By A. J. Taylor, F.L.S., F.R.G.S. Eng.

12. "Notes on the Shell Mounds at Seaford, Little Swanport."

COUNCIL.

No change has taken place during the session.

LIBRARY.

The most noteworthy circumstance in connection with the Library has been the completion of the rare and expensive work "The Birds of Asia," by J. Gould, F.R.S., the remaining 23 parts being a munificent gift to the Society by the Hon. J. W. Agnew, M.D.

FELLOWS.

Six Fellows and two Corresponding Members have been elected; twenty have been lost through resignation or death. The hearty congratulations of the Council were, during the session, given to Mr. James Barnard and the

Hon. Dr. Agnew, who in May celebrated their jubilee as Members of the Royal Society of Tasmania and its precursor, the Tasmanian Society for Natural Science.

FINANCE.

The income has been :—Subscriptions to Royal Society, £175; fixed deposit, £208; interest, £10 18*s.*; total, £393. Expenditure, £235 1*s.* 1*d.*

In moving the adoption of the Report the Chairman stated that the meeting would have been held in January but that the meeting of the Australasian Association for the Advancement of Science took up the whole of the time of those engaged. The Society had met with great success, for which the thanks of the Members were due to the liberality with which their requests had been met by the Parliament of Tasmania. The Municipal Corporation also deserved thanks for the manner in which they had given space at the Town Hall to hold the lectures in. The Committee had used almost superhuman exertions to make the affair a success, and from the results which had attended the labours of those engaged there was no doubt but that the whole had been a most undoubted success.

Mr. C. H. Grant seconded this, which was carried.

Votes of thanks were moved by Mr. C. Belstead and seconded by Mr. C. H. Grant, to the Auditors for the work carried out by them during the past twelve months, they being re-elected to fill that position; and by Mr. A. Morton, seconded by Mr. C. H. Grant, to the Press as follows :—" That a vote of thanks be accorded the Press for its valuable services in promoting the objects of the Royal Society by its ready and accurate reports of the evening meetings, and in various other matters,"—both of which were carried unanimously.

Mr. Morton, in moving the above, spoke in feeling terms of the services rendered by the late Mr. J. Johnson, formerly a member of the reporting staff of *The Mercury.* The seconder and the Chairman also endorsed the remarks made by the Secretary, after which the meeting terminated.

STATEMENT of the Funds of the ROYAL SOCIETY *for Year ending* 1891.

Receipts.	£	*s.*	*d.*	£	*s.*	*d.*
Balance *from* 1890	28	12	0			
Fixed Deposits	208	0	0			
				236	12	0
Subscriptions		...		175	10	0
Refund Weaver's Cheque		...		1	1	0
Interest on Fixed Deposit	10	18	0			
Ditto	10	8	0			
				21	6	0
Bank Balance per Pass Book Credit	5	19	1			
Interest drawn in error	0	5	0			
	6	4	1			
Outstanding cheques £9 6*s.* 8*d.*; £15 17*s.* 6*d.*	25	4	2			
				19	0	1
				£453	9	1

Expenditure.	£	*s.*	*d.*
Salary of Secretary	50	0	0
Ditto Attendant	6	0	0
Current Expenditure	179	1	1
Fixed Deposits	218	8	0
	£453	9	1

Examined and found correct.

JOHN MACFARLANE.
FRANCIS BUTLER.

29th March, 1892.

STATEMENT of the Funds of the MORTON ALLPORT MEMORIAL FUND *for the Year* 1891.

1891. Dr.	£	s.	d.	Cr.	£	s.	d.
Balance as per statement for 1891..............	68	1	4	Purchase Buller's "Birds of New Zealand"	13	13	0
Interest	2	14	6	Balance in Savings Bank to credit of Fund	57	2	10
	£70	15	10		£70	15	10

Examined with Savings Bank Book, and found correct.

JOHN MACFARLANE.
FRANCIS BUTLER.

29th March, 1892.

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